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CLINICAL REMARKS

ON CASES AT

ST. GEORGE'S HOSPITAL,

BY DR. SEYMOUR.

SESSION 1842-43.

LEPRA VULGARIS AND ICHTHYOSIS.

BATEMAN divides cutaneous diseases into four distinct classes. In the class *squæ* are included lepra, psoriasis, and ichthyosis. We have a patient up stairs named Clapham, who has got *lepra vulgaris*. He has had it for five years. It has been generally supposed that long duration renders this disease more difficult to cure. But from experience I am inclined to think that the very contrary of this is the fact. The disease consists of raised scales, spread over a slightly reddened surface, generally showing itself about the region of the articulating surfaces. I believe that it never occurs about the face and hands; it shows itself in circular patches, which are distinct and isolated from each other, and possess a slight degree of cuticular irritation. As time advances, however, these circular patches spread, increase in their diameter, and form, as they have done in this case, a circular patch, around the arm affected, making it very stiff. Where the scales are smaller and whitish, the disease has been termed *lepra aphthosa*, but this is only a variety, requiring the same treatment. As the disease advances these raised scales fall off, leaving the skin underneath very red and tender. Lepra does not affect the general health, nor does the constitution affect lepra. The best remedy is, undoubtedly, arsenic. How it acts on this morbid state of the skin is, however, quite unknown. It gradually removes the scales, leaving nothing but a slight blue mark on the skin, as in the man Clapham, who is going out with nothing but the peculiar blue appearance on the skin where the leprous spots were situated. The arsenic was combined with the decoctum dulcamaræ, a medicine which is also said to have a very great effect on this disease, first brought to notice by Sir Alexander Crichton. I have known the arsenic and the dulcamara fail over and over again, when exhibited singly. I have experienced only one exception to this; but I have never known these two medicines in combination to fail. In addition to the arsenic and dulcamara, internally, this patient was also ordered to use the latter medicine as a lotion to the parts. I believe this to be very useful, as it produces some irritating effect even upon healthy skin. Some time after commencing to take the arsenic it was stopped, and he was bled, for this reason: when given for a length of time arsenic may cause severe pain in the stomach, and affect the head. If the former you may be pretty nearly sure that the medicine is producing its constitutional effects. But it is quite certain that arsenic will also produce injurious effects upon the head, and whether the stomach or the brain be affected, its exhibition should be suspended until these symptoms pass off. In exhibiting arsenic I should advise you to give it for a fortnight, and then stop for the same period, and in giving it for lepra you can during its omission still continue the dulcamara. I said just now that I only knew of one case of lepra in which the arsenic and dulcamara had failed, and that was in a boy whom Dr. Page had put under the same remedies without good effect. He was admitted here, and I continued them, supposing that good board and lodging in the house might increase the efficiency of the medicine, but I was disappointed, for no benefit followed its exhibition. He afterwards took sulphur, but became much worse under its use, and his arm assumed the appearance of ichthyosis, not, however, of that aggravated form which you see depicted in some representations of cutaneous disease, but the leprous scales ran into one another, and presented no definite shape. The disease had now, therefore, assumed a new type; the lepra was lost in a mild form of ichthyosis, and as in this latter disease pitch has been said to be serviceable, I gave him four or five grains three times daily, gradually increasing the dose until he took twenty-five grains three times daily. Under this treatment his health grew better, and the disease gradually disap-

peared. He continued the medicine for two months, and has continued well ever since.

PSORIASIS

is more difficult to manage than either of the preceding diseases. This also is a scaly disease, but the scales are not disposed in circular patches, but in larger, undefined, masses. After a time it is attended with an intolerable cuticular irritation and distress, whereby the general health may become affected. When it appears late in life it often proves valuable as a revulsive to some internal disease, which lepra never does. The sub-cuticular surface beneath the scales is very tender, and frequently discharges copiously.

POMPHOLYX.

Bullæ is another class of cutaneous disease mentioned by Bateman. It includes erysipelas and pomphosis, which latter consists of large bullæ, attended with very severe smarting. I have never seen this peculiar form of disease, and it is very rarely to be seen in this country. But of another cutaneous affection included in this class, I shall now speak, and that is pompholyx, which consists of small bullæ rising suddenly over the whole surface of the body. Such cases are by no means uncommon; I remember a most remarkable one where the patient took arsenic, but with little benefit, until a slight mercurial was added, half a grain of calomel night and morning. Mercurials, in all forms, had been tried before, but by themselves. Arsenic had also been given by itself, and so had a number of other things. But the combination of the calomel and arsenic did succeed, and in all the cases that I have seen I have observed that medicines in combination act with greater power and efficacy upon the disease than when taken in an uncombined state.

PORRIGO.

The next class of cutaneous diseases of which I shall speak is the Pustulæ, and the first affection under this head is porrigo, of which there are two species. It consists of pustules which arise at the roots of the hair, in consequence of which the hair falls off in patches. The most common form is porrigo scutulata, or ringworm, in which the hair is succeeded by a dirty crust. Different appearances are assumed by this disease, causing numerous subdivisions of it. Thus, where the crustaceous deposit is extensive, and gives rise to a yellow discharge from the surface, it has been called porrigo favosa. Where the hairs are matted together it has received another name. But all these are only varieties of the same disease, depending very much upon a certain want of cleanliness or other circumstances, for which the patient is perhaps more answerable than the disease. In this other species of the disease, porrigo decalvans, as I have said, the hair falls off in patches, and leaves a clear free surface, unmarked by any pustular appearance. Many of you recollect the child in Crayle ward, who had several of these patches about the head, and at the summit of the scalp there was one pustular patch of porrigo scutulata. I think the best remedy in such cases is aromatic vinegar, applied to the affected parts night and morning until they become very sore, when it should be left off. Now, this child was so treated, and it is astonishing how rapidly the good effect was produced. The hair is now growing over all the patches, excepting over the one where the disease assumed the form of porrigo scutulata. Before this child was admitted she was in great want and poverty, but I do not think that bodily impoverishment has anything to do with the disease, nor that internal remedies are of avail in any form of the disease. Innumerable applications have been recommended for the external treatment of porrigo scutulata. The great secret of all cure in this complaint is to cause as much irritation over the diseased surface as it will bear, keeping a moderately caustic application constantly to it for a very long time. What the peculiar application should be does not much signify, but you may employ a strong solution of the nitrate of silver, or galls, with ointment of cantharides and verdigrease; but whatever caustic you use the principle of treatment is the same in all. Porrigo larvalis, or crusta lactea, occurs in very young children, and generally in those of a very scrofulous habit, showing itself in the forehead and face as well as on the head, not in little patches only, but it spreads equally over the skin, the matter poured out being very offensive. Should it break out during teething the child is generally much relieved by it; but if suddenly checked effusion into the brain and other serious effects may be caused. Never attempt to cure this disease without attending to the digestive organs. Give the child mild mercurials, combined with sulphur. The parts affected should be well washed, and kept quite clean with gruel, but never try to dry up the discharge suddenly, or you will bitterly repent it.

ACUTE RHEUMATISM.—RHEUMATIC FEVER.

There are two cases of this disease in the hospital under my care. It is characterised by great redness and swelling, affecting the muscles and ligaments of the part which is pained and swollen, generally one of the larger joints. There is a very quick pulse, a hot skin, and a white furred tongue. The pain is generally, but not always, relieved by warmth. When not so relieved it is often accompanied by profuse sweats breaking out about the affected joints, but which do not relieve the pain. The urine is thick, and deposits a copious sediment, which is always acid, and generally composed of the

lithate of ammonia. The perspiration poured out over the affected joints is also of an acid character, and is sometimes mixed with an earthy secretion. The saliva also is acid. All the excretions eliminated from the blood are frequently acid, while the blood from whence all these secretions are derived is found to possess no acidulous properties. It is an alkaline fluid, and if any acid be mixed with it it must be by being injected. The pain may shift from one limb to another. If this affection be allowed to run on uncontrolled, it will last from eight to ten weeks. I know a surgeon, in extensive practice, who has had much experience in this disease, who declares that do what you may, and with every drug in the materia medica, you will never find an attack of acute rheumatism last less than six weeks. Disputes and theories ran very high in former times as to whether this disease was inflammatory or not. You will always find that the blood has an inflamed appearance, but it is a curious fact that if you do nothing but bleed the patient, you may bleed him to death, and the last drop of blood will be as buffy and inflammatory as the first. The excessive pain of this disease depends on the affected textures being principally ligamentous, and, therefore, when inflamed, highly susceptible to pain, and if any muscular structures be implicated, every twitch of the limb affected tends to increase the agony. One of these patients was bled on admission, and my experience in this disease leads me to say that I have never omitted bleeding in the outset of the disease without having had great occasion to repent it afterwards. Remember, however, that you must not go on bleeding in this way. I have seldom or never had occasion to repeat the bleeding. After this man was bled he was ordered the guaiacum mixture every four hours, and one grain of opium at bedtime. The latter is a very beneficial medicine, as it procures sleep for the patient, and prevents the medicine from running off by the bowels. I have employed this peculiar treatment in rheumatism ever since I was a student, and with such success that I have never seen reason to alter it. This man's case was a very bad one; yet in a week's time all trace of rheumatism had left him. With respect to the action of the guaiacum, I believe that it increases the secretions, but that is all that I know of it. If given uncombined with any other medicine it acts very copiously on the bowels, and produces a copious diaphoresis all over the body, not that partial diaphoresis which I before spoke of as aggravating the sufferings of the rheumatic patient, but a general diaphoresis, which eventually tends greatly to his relief. I was first led to give the guaiacum mixture in rheumatism whilst a student at the Royal Infirmary in Edinburgh. In this institution there was, of course, a very large proportion of cases of rheumatism, which were treated by different methods in order that the students might contrast one method with another. The late Dr. Home always treated rheumatism with guaiacum, and I remarked that they all rallied much sooner than under any other method. With respect to the relief which a rheumatic patient receives from copious perspirations, I must tell you that these are not to be produced by any means which you may employ. You must especially guard against the very hazardous method of employing warm-baths for the purpose. I am acquainted with no plan of treatment which is so likely to prove injurious to the patient as this. But you may naturally ask me whether there are no other means of treating rheumatism. Oh yes, there are. Some persons treat rheumatism with calomel and opium, and cure it by those means, but not, I think, so speedily as by the plan which I have laid down, certainly not more speedily, whilst that mode is attended with the evident disadvantage of making the patient's mouth sore. I know many sound practitioners who treat their patients successfully by giving them calomel and opium, when affected with rheumatism, but I should ever prefer the treatment by bleeding and guaiacum, were it only on account of the one result, ptyalism. Many ladies will very obligingly take calomel until their mouths are sore, and their rheumatism is cured, but when their mouths still remain under the influence of mercury, they will forget the cure you have performed, and perhaps, also, in consideration of their mouths, forget to send for you again. You will also hear that colchicum cures rheumatism; not, however, in this form of the disease, but only where a joint, independently of the surrounding textures, is affected. In such cases it acts almost magically,—in proportion, in fact, to the amount of effused fluid. But you will sometimes be called in to milder forms of the disease, which you will be able to cure by administering salines, antimonials, and Dover's powder. Then, again, you will meet with cases in which the cause has been of very long duration, and there is excessive pain, attended with muscular twitchings, which harass the patient night and day; in addition the affected parts will be exceedingly swollen, but not red, and there is that excessive local diaphoresis of which I have before spoken, but which affords no relief. Now, this is a very troublesome rheumatism to treat, and sometimes will defeat every remedy, though I believe that opium, one grain every six hours, is the best medicine in these almost intractable cases. Be careful, however, to keep the bowels open whilst giving it. It happens, however, in some cases of rheumatism during the course of the disease, or as I believe, simultaneously with its origin, that the patient complains of pain about the heart, and the

pulse will be hard, small, and quick, and soon become irregular. The patient can lie on his back, and, in the earlier stage, may be able to rest on the affected side, but his countenance will become indicative of great anxiety and depression. These symptoms denote inflammation of the pericardium, and although, on its accession, the rheumatic pain leaves the extremities and centres about the heart, yet it sometimes does not do so. Now, if this goes on unchecked, in six or seven days your patient will die, and sometimes sooner, and during the last few days he will be tormented with dyspnœa, constant cardiac pain, and frequently-recurring syncope. After death you will find the opposing surfaces of the pericardium covered with lymph, with much effused serum, and globules of pus floating here and there in the pericardiac cavity. If the rheumatic affection of the pericardium has continued for any length of time, there will be found successive layers of deposited and organised lymph, presenting a reticulated appearance. The opposite surfaces of the pericardium will be adherent. This, I think, occurs universally. The heart thereby becomes impeded in its action, and enlarged in volume; dropsy supervenes, and the patient dies. Suppose you are called in to a case of this kind; first you must bleed, then give calomel and opium as soon as you can. Indeed, it is astonishing what very large quantities of calomel you may give in this rheumatic affection of the heart, without causing salivation, or any perceptible effect, excepting on the disease itself. This mode of treatment is not a specific, for it does not *always* succeed, though almost always, and I have generally found it to be most successful in proportion as the attack is acute, and I have seen case after case of this kind recover, so that the patients, after a certain time, have gone about their ordinary occupation. But still, though the treatment be thus far successful, the heart remains more or less crippled, and, sooner or later (for years may elapse), the fatal results I have spoken of are sure to occur. I must tell you that many consider that bleeding conduces to this after attack of the heart, but I am myself most decidedly opposed to this opinion, and I do not remember a single patient whose treatment I had from the beginning, in whom the heart became affected, though I have seen great numbers come into the hospital with the heart in this state.

EPILEPSY.

This peculiar affection may be said to be of two kinds. In the one the patient has an undefinable warning of the approach of the epileptic paroxysm, sometimes like a creeping sensation, running up from the extremities towards the head, and termed the *aura epileptica*. Some persons, however, have nothing of this, but, instead of it, an undefined, unfixed pain in some part. Soon after, and frequently immediately after the occurrence of these symptoms, the "fit," as it is termed, comes on, and the patient falls down, perfectly insensible, struggling violently, in many cases foaming at the mouth, and breathing with the greatest difficulty. These frightful symptoms having continued for a space of time, the sufferer lapses into a profound sleep, and on awaking he feels nothing but a sensation of weariness, and in this way he may go on for a longer or shorter time until another fit ensues. The second species, which appears to be slighter in its effects, and milder in its phenomena, although certainly not less dangerous, is where the patient merely loses his sensibility for a minute or two. I need not here enter very deeply into the pathology of the disease, but I may say that epilepsy is very often connected with, or dependent upon, certain lesions of the cerebral masses, such as tumours, or abscesses in, or softening, most frequently situated at the base of the brain, or the cerebellum. Sometimes they may be the result of blows, and years may elapse between the injury and the manifestation of its effects upon the nervous system. In some severe and protracted cases of epilepsy you will find no appearances in the brain after death to account for the disease; whilst in others it is the result of a nervous sympathy between the brain and the remoter parts of the nervous system. For instance, I have known the passage of a bougie through a stricture of the urethra bring on an epileptic fit. In women, again, any uterine disturbance will produce it. For the relief of this disease all sorts of empirical preparations have been recommended as successful, but for what particular case of epilepsy each nostrum is fitted it is very difficult to say. Those medicinal roots which are said to contain a fetid principle have been highly praised. I have seen some cases, depending greatly upon uterine disturbance very much relieved by the oil of amber, and others by the nitrate of silver, but the latter darkens the skin. The case on which I have founded these remarks is one of five years' duration, but that is no reason for supposing that it depends on organic lesion of the brain. The man took the sulphate of zinc, which I have often given, but never saw it do much good, perhaps because it was not used long enough; but a late number of the "Guy's Hospital Reports" contains a paper on Epilepsy, by Dr. Babington, who states that in many cases he has known this medicine to be eminently successful. I recommend this paper to your notice. Dr. Babington is a sound physician, not one of your new-fangled-doctrine men, and therefore his opinion is valuable.

THE cause of stiffness after death is the coagulation of the fluids of the body, particularly the fibrin of the blood.—*Brucke.*

POISONING BY ARSENIOUS ACID
ULTIMATELY DETECTED BY
EXAMINATION OF THE LIVER.

To the Editor of THE LANCET.

SIR,—Having been for some years past an attentive reader of your valuable Journal, and having received much pleasure and instruction from its perusal, I consider myself bound to communicate to it, in return for my enjoyment, what little information it may be in my power to contribute.

The case which accompanies this note was to me highly interesting, and presented several facts that were worthy of reflection. Perhaps the most important is the discovery of arsenic in the liver, in great abundance, after numerous experiments upon the stomach and its contents had failed to detect it; thus illustrating the important medico-legal fact, first promulgated by Orfila, that this poison *often disappears from the stomach by absorption, and becomes separated from the blood of the portal system by the liver*, and that it might be detected there even after all evidence of its presence has elsewhere vanished. I regret that no opportunity occurred of examining the urine, since the bladder was empty; but evidence existed of inflammation of the mucous membrane of the pelvis of the kidney and ureters. Could that have resulted from the action of this poison? Believe me to be, Sir, your's most obediently,

WILLIAM BIRD HERAPATH.
Pupil of the Bristol Medical School.
Old Park, May 10, 1843.

Post-mortem appearances. The stomach and its contents; various white matters found therein; great inflammatory action on its mucous surface. Effusion of coagula. The ovaries encysted, and the Fallopian tubes closed and dilated into serous sacs. Local inflammation of the peritoneum. Fluidity of the blood in the heart's cavities. Congestion of the lungs. Chemical experiments on the fluid contents of the stomach, on a portion of that viscus, and on the various white matters and specks found in it, and upon the blood of the right side of the heart but little arsenic found so far; arsenic found in the liver, in much greater quantity.

June 14th, 1842.—Jane Burns, ætat. 31, having died suddenly at her lodgings, and under suspicious circumstances, Mr. Grindon, the coroner, ordered a post-mortem examination of the body to be made. Mr. Ormerod, having been the medical man in attendance upon the deceased, was instructed to perform it, and having called upon me to conduct the chemical examination of the case, I requested to be present at the opening of the body, which we found in an upper story of a lodging-house kept by a Mrs. Brown, living in Newfoundland-street, St. Paul's. Its exterior presented no marks of violence; it was much discoloured in the dependant portions by the gravitation of blood. It was fat and plump, and in very good condition, and presented the appearance of a party dying in the prime of life. Death could only have recently taken place, since the limbs were still rigid. The body was opened in the usual way, and the contents of the thorax and abdomen were exposed.

The lungs were full and prominent; they did not collapse upon allowing the pressure of the air to exert its influence; not emphysematous; no tubercles seen, if we except a small cretaceous one in the base of the lower lobe on the left, the size of a pea. The lungs were engorged with blood almost as if asphyxia had caused death; upon cutting into them it flowed out in quantities. The black pulmonary substance was deposited in its usual situation; two or three small black bodies, as large as sparrow-shot, were seen in the interlobular fissures, probably enlarged lymphatic glands blackened by this substance.

The pericardium contained only a teaspoonful of serum.

The heart large and flabby; right auricle and ventricle distended with black fluid blood; this was collected for experiment, about six ounces were obtained; the left cavities also contained blood, but not to the same extent, this was also preserved with the other; no coagula on either side. Valves: lining membranes all healthy.

The pleuræ contained several ounces of serum, but this was, through an oversight, not saved for examination.

The abdominal cavity and its viscera.—The peritoneal surface of the small intestines had a beautiful pink tinge, similar to the hue of a maiden's-blush rose; several vessels very distinctly marked. Omentum fat, but healthy. Upon pulling up the small intestines (a few coils of the ileum) from the cavity of the pelvis, several bands of organised false membranes were found connecting them with the uterus and with each other; these bands were highly vascular and organised; the peritoneal disease was local, extending only to about the five or six inferior convolutions of the ileum, and were consequently found on the right side. The cœcum was *not* affected in this way, nor its appendix. Other intestines healthy in external appearance.

The stomach was next removed, having previously placed ligatures upon its œsophageal and pyloric portions, removing about an inch of the œsophagus and duodenum at the same time. Upon opening this organ it was found to contain about half a pint of thick grumous fluid, yellowish-red in colour, some half-digested portions of food in it, too much altered to be recognised. These contents were preserved in clean bottles and

sealed. The mucous surface of the stomach presented several appearances worthy of notice; its greater cul-de-sac, and a portion of the greater curvature, had a dark livid-red colour; upon closer examination, a clot of blood was seen to have been effused upon its surface and also beneath the mucous membrane. The rugæ were very distinctly marked, and many of them deeply injected and highly inflamed; its pyloric portion was scarcely affected, but the mucous follicles were enlarged, and numerous *white specks* were sprinkled over its surface. Upon raising the coagulum, the mucous membrane was highly inflamed and much softened, being easily torn by the nail. Throughout every part of the stomach these white specks were seen, but they were more numerous in the foveolæ or pits between the rugæ. Some *patches* of white matter were observed upon the coagulum before spoken of, some were also scattered upon the surface of the lining membrane of this organ; but a *peculiar white, shining*, (apparently) crystalline body was seen everywhere diffused. (There was another white body present, but this was *not seen* at this time.)

The stomach was now carefully placed in a new bladder, and reserved for re-examination at more leisure.

The duodenum was removed and examined, but contained only a little fluid, and its appearances were not much worthy of remark. The jejunum contained some fluid and gas; about five ounces were preserved like those of the stomach. One or two red spots upon its mucous membrane, but considered unimportant. The ileum was not examined through its whole length; those portions which were opened presented little worthy of remark, even the membrane of that part whose peritoneal coating was so inflamed was perfectly healthy. The large intestines not minutely examined. The liver was gorged with blood, but healthy in structure; about half a pound was preserved for subsequent examination. Soon after this I found upon my finger a distoma hepatica, or fluke, it was still alive, and had the usual greyish colour of those found in the liver of the sheep; no others could be seen.

The pelvic viscera were next examined. The finger inserted into the vagina reached the neck of the uterus without difficulty; it was large and well-marked; the os tincæ large and elongated; although not pregnant, she might have had a child; she could not have had *more*, as the skin of the abdomen presented none of those seams so well known as the results of pregnancy.

The uterus was of the normal size and unimpregnated; upon its exterior the remains of the false membranes were found which had connected it with the convolutions of the ileum. The Fallopian tubes and right ovary were rather remarkable, and appeared as if they contained an extra-uterine conception. The whole contents of the pelvis were, therefore, taken out with care for closer examination. The bladder was empty. This was to be regretted, as valuable indications might have been obtained of the presence of arsenic in the urine. The kidneys were congested with blood, and the calyces contained some muco-purulent fluid, and apparently the mucous membrane was inflamed, as it was highly vascular. Was this the result of gonorrhœa or of arsenic? no other evidence of the former disease existed. I am inclined to the latter opinion, but cannot prove its truth.

To return to the genital organs. The uterus upon being opened was healthy and empty; the mouth admitted the handle of a small scalpel easily. The Fallopian tubes were both encysted, the fimbriated extremities being adherent to the recto-uterine fold of peritoneum on the left, but to the enlarged ovary on the right. Their osteo-uterina were obliterated, the adhesions were probably old, as they were organised. The right tube being opened, about two or three drachms of yellow serous fluid escaped; very little fluid flowed from the left, scarcely a drachm, but no fœtus in either. The left ovary contained several cavities; one seemed to communicate with the peritoneal cavity, as an opening was found having rounded edges and not appearing as if cut, this cavity would have lodged a pea; the internal surface was slightly yellow; all the sacs were empty and communicated; three or four were found.

The right ovary was at first, from its feel and appearance, supposed to contain an extra-uterine conception, and it was opened rather reluctantly; it evidently contained a fluid and a solid; this was rounded and not well defined. Upon making a puncture, about two ounces of reddish serum (sero-sanguineous fluid), escaped, and a solid body with a shaggy surface was seen blocking up the orifice; this was, therefore, enlarged and the body turned out; it was found to be a coagulum of blood (?) homogeneous in texture, but yellowish within. The walls of this cyst were lined by a false membrane, upon the removal of which the serous shining surface of the cyst appeared. The whole ovary was as large as a tennis-ball. The peritoneal coat presented the remains of the false membranes attached to it, thus holding the ileum down firmly, as before stated. The walls of the ovary were only one-sixteenth of an inch thick, and *none* of the healthy structure of the ovary remained.

Did the peritoneal inflammation result from the rupture of a similar cyst in the left ovary and the escape of the fluid into the peritoneal cavity? I am inclined to think that it did. It is clear that she could not have been pregnant, and doubtful if she ever would have been again, had she lived.

The woman was a native of Ireland; her maiden name was Maggs. She married a man named Wright, who died; she then married a serjeant (now in India), but she formed a disreputable connection with a man living at Cathay, and was in the habit of meeting him every Saturday night, at a public-house, and he then usually accompanied her home; but on the 11th instant they quarrelled, and he did not go home with her. On Sunday morning she was found ill, having previously written some letters with pen and ink borrowed from her landlady, together with a prayer-book; some time after this a little girl was sent up stairs to fetch these things back and found the door locked on the inside. By-and-by the landlady heard a noise up stairs, and found the deceased very ill, and groaning terribly. She fetched her neighbour, and then the woman asked for water, and soon afterwards was sick, and "vomited a large quantity of white stringy fluid," which they foolishly threw away. To a medical man who was sent for she complained much of her heart, and appeared in great agony, rolling and tossing in her bed, but he could gain very little information from her. She either would not or could not answer his questions. Before any medicine could be prepared she died. In her room he found in her reticule a small paper packet (with "arsenic, poison," written on the outside), containing a white powder.

She had only been in the house a week, and upon taking the lodgings had stated that her husband lived in Bath, but came home on Saturday nights, and would then pay the rent. She added that she believed herself to be *enciente*, but the post-mortem examination clearly proved that this suspicion was unfounded. It was evident that she could not have been recently delivered. It is doubtful whether she *ever* had a child, although the uterine orifice was not in its virgin state. The disease in the ovaries and Fallopian tubes would prevent her from becoming pregnant. Although it is not very probable that this was a congenital malformation, yet it must have existed for some time, perhaps for several years. The peritoneal disease must have been old too, since the membranes were organised, and no recent lymph or fluid was effused.

The congested state of the lungs and the engorgement of the right side of the heart, would seem to point to asphyxia as the immediate *precursor*, if not the *cause*, of death.

But the gastric symptoms must have been acute with such extensive disease; the inflammation was intense, blood and coagulable lymph having been effused, together with softening of the mucous membrane, in patches, all being evidences of acute gastritis, and appearing to point to a corrosive poison as the proximate cause. The abundant presence of white particles, &c., in the stomach seemed to support this conclusion.

Under these circumstances I commenced the chemical examination with alacrity, expecting to find it in abundance, and in every portion used.

I first assured myself, by reducing the metal, that the substance *contained in the paper* found in her reticule was arsenious acid; again oxidising it, and obtaining the green arsenite of copper, the yellow arsenite of silver, and the yellow sulphuret of arsenic.

A. I took a portion of the fluid contents of the stomach. About four ounces were evaporated to dryness (during the boiling a strong smell of toasted cheese was perceptible); it was next mixed with nitre, and deflagrated in a pure silver dish. When all the animal matter was destroyed, and the dish and contents allowed to cool, it was treated with water until dissolved, then filtered. To the clear solution acetic acid was added in small portions, until a slightly acid reaction appeared evident to litmus-paper, carbonic acid being evolved; a current of sulphuretted hydrogen gas was then passed through this. A brownish-white precipitate was thus obtained. The fluid was now boiled to expel the excess of hydrosulphuric acid, and to assist the coagulation of the precipitate. It was now set aside, in order that the precipitate should fall. Next day the supernatant fluid was removed by the pipette, and the precipitate dried. It was then treated with a little water, and got into a smaller compass, and again allowed time to subside. This water was again removed by the pipette, and I again dried the precipitate. Having now well dried some charcoal and carbonate of soda, small portions of these substances were mixed intimately with half of the precipitate, and introduced into Berzelius's reducing tube by means of a small funnel, and then, upon cautiously applying heat, so as to expel a small portion of water (which always remains notwithstanding all your care), and having removed this by means of a small coil of bibulous paper introduced into the tube for that purpose, the bulb of the tube was heated to bright redness; a little sulphur rose or sublimed. Beneath this ring of sulphur the merest trace in the world of a *dark ring* was perceptible, but this was not sufficient to be able to say that it was arsenic, although a *very suspicious* circumstance. (I preserved this.) The second portion, treated in the same way, gave similar results. Upon endeavouring to pass this portion through the usual series of tests for arsenic in solution no results were obtained.

B. In the intervals occurring in the progress of this experiment I took about two or three inches square of the *pyloric extremity* of the stomach (here, be it observed, the white specks were most numerous) and dried it in an oven, having previously

washed its surface, to obtain the white particles. The washings were evaporated to dryness, and treated precisely in the same way as the last, but not the slightest trace of arsenic was perceived.

C. The solid portion of the stomach from B was treated exactly in the same way. The residue was subjected to the same process as in A: a slight smoky tint only obtained by reduction, but *far* from being conclusive. I preserved this as a specimen.

D. During the spare time, whilst these experiments were on hand, I took four or five ounces of the blood, evaporated to dryness, constantly stirring it, and treated this by the same process. Had I been able to foresee the *results* of these experiments I should not have tried this, as these were still *in umbrâ*. I commenced this merely as an experiment from curiosity, as all the previous experiments could not be finished until the next morning. This was treated precisely as before, and in the morning it was reduced, but nothing rose, a ring of sulphur only subliming.

E. Having so signally failed in all these trials I began to feel annoyed; for being morally certain that arsenic had caused death, why was it not found? The process had proved successful over and over again in my hands, and my father has been eminently successful with it. Then why was it fallacious here?

I now searched for other poisons, and as the mineral acids were likely bodies, these formed the indices for experiments, but none were present.

Then it occurred to me to examine more closely the substances still adhering to the surface of the stomach. Had all these white specks, &c., been arsenic, not the least difficulty could have happened with regard to them.

First. Multitudes of small, white, shining, crystalline globules were present, but of a very *small* specific gravity, and having a silky lustre. Placed on a Stanhope lens they were very transparent, and not unlike granules of starch; but tincture of iodine gave no indication of that substance. They were insoluble in acetic acid; ammonia, boiling, partially dissolved them, as did potash. Were they fatty grains? I think so. They were everywhere seen upon the stomach, more in the pits than on the rugæ, *floating on the fluid*.

Second. A more opake white substance, found near the pyloric end, more like arsenious acid, but when placed on the Stanhope lens they proved to be soft and plastic, were not gritty between the nails; when placed in a small tube and heated they intumesced, charred, and smelt strongly of toasted cheese. A crystalline white body subliming, this was not soluble in water, and the water, when tested for arsenious acid, gave a *blue* precipitate with the ammoniacal sulphate of copper; they were not arsenic, neither did they contain any.

Third. A third white substance; white patches: these were apparently fibrinous. Took a portion of the coagulum with these patches upon it, boiled in strong nitric acid, again and again, until no red fumes of nitrous acid evolved, then evaporated to dryness. A fatty body left; upon adding ammonia, an orange, soapy compound; this was then treated with acetic acid and sulphuretted hydrogen, as before, the precipitate attempted to be reduced, but nothing sublimed.

Fourth. I scraped a portion of the mucous membrane with a spatula; the mixed bodies, &c., were acted on by nitric acid, as in the last experiment, and neutralised by ammonia. Upon dissolving by the aid of heat, a yellowish soap, as before; upon testing this solution by the copper, silver, and sulphuretted hydrogen tests, something like the precipitates were obtained. However, as these could not be depended upon, as the animal soap was present, this was deflagrated with nitre, as in A (after previous evaporation to dryness), and being treated as before, nothing rose.

Fifth. I was now obliged to attend the inquest. Having only suspicions of the presence of arsenic, I gave my evidence in a guarded manner, and having detailed these results, requested an extension of time; but the coroner and jury thought, notwithstanding my having stated my belief that it was present, that nothing would be gained by the delay, especially as the evidence all tended to prove that she had committed suicide. They therefore returned a verdict of died "from inflammation of the stomach." Upon my return home I again set to work, and having poured a fine stream of water upon the stomach so as gently to wash off these light particles, so embarrassing, I took it into the sunlight, and carefully went over the whole mucous membrane (of the stomach) with a lens, inch by inch. By this means several small grains of a white body were obtained, very different from those previously found, appearing as opake white specks, very sparingly scattered over its surface and closely adherent to it. Their margins were more defined than any yet seen, its edges being sharp. Being observed amongst the patches of different white substances previously found, they stood somewhat boldly forward, inviting attention. Having, after a good deal of trouble, picked out with the point of a pin and the help of the lens about twenty or thirty of them, placing them one by one, as I did so, in a watch-glass with a little water, some fell down, as heavy bodies would do, others swam and floated away. At length, having sufficient of the heavy ones, I decanted the fluid and lighter particles, washing those remaining in the watch-glass again and again with a little

cold water. They were then dried and mixed with carbonate of soda and charcoal, as before, and having introduced the mixiure into a reducing tube, upon heating it in a short time the metallic arsenic arose in its usual splendour. This I preserved. I again went over the stomach, and obtained a second portion of these particles. Some were preserved as specimens, others were reduced and then subjected to the usual tests, as additional evidence of their nature.

F. Not being very well satisfied at having missed this body in the fluid contents of the stomach, I now took the remainder of the contents of the stomach and intestines, about two-thirds of the original quantities found; they were treated as in A. Upon subliming this, I obtained a *slight* indication of metallic arsenic, a ring of fly-powder appearing; the metallic ring *was not* perceptible; it would do as a corroborative evidence only.

G. Took half a pound of the liver; having cut it into small portions and introduced them into a tubulated retort, they were boiled with concentrated nitric acid; violent effervescence took place from the escape of carbonic acid, nitrogen and nitrous acid gases, more and more acid being added from time to time; the fibrinous masses first disappeared; being converted into a fatty substance, this gradually disappeared also, leaving a deep orange-coloured solution; this was evaporated to dryness, and the operation again recommenced as before. When the decomposition had been pushed as far as possible without danger of the explosion which so often occurs towards the end of this process, it was stopped by slow evaporation to dryness again; the residue was dark chocolate-coloured, and weighed about two drachms.

H. This residue was now boiled in a solution of carbonate of potass; effervescence took place, an orange-yellow solution being formed, and the same dark-chocolate residue remaining. This was proved to be sesquioxide of iron with animal matter.

I. The solution was now separated by filtration, and slightly acidulated with acetic acid and a current of sulphuretted hydrogen passed through it; then boiled to expel the excess of this gas and allowed to stand for a time in order that the precipitate should subside; decanted the fluid and washed and dried the precipitate; part of this was now introduced into the reducing tube, as before, when upon heating it an *abundant ring* of metallic arsenic sublimed. This was preserved as a specimen, an other experiment gave similar results, and farther, for by oxidising the ring of metal and dissolving in water and applying the tests for arsenious acid in solution, the characteristic precipitates were produced.

K. The residue from H, upon being farther examined, was found to contain no arsenic, but consisted of phosphate of lime, sesquioxide of iron, animal matter, and a portion of silica.

This operation, namely, the destruction of the animal matters of the liver by means of nitric acid, is a long and tedious process; yet it is an interesting one, and, by its results, fully repays one for the trouble, for in this case we see what minute portions of the poison remained in the organs where it is usually sought for. The *fluid* contents contained the merest possible trace, certainly not near enough to enable one to speak positively of its presence, although it is possible to prove the existence of one-thousandth part of a grain of this substance, and very probably even a much less quantity. It is true that by *great care and trouble* several little granules of the poison were obtained *from the coats* of that viscus, but the operation takes too much time to have allowed me to use this process before the inquest. Again, the *best* plan would have been, undoubtedly, to have destroyed the whole stomach by the and of B; but the case appeared so clear, process such masses of evidence seemed already present, that this caution was apparently unnecessary.

The discovery of arsenic in the liver, when such small traces exist in the stomach and its contents, appears to be important; highly so, indeed; in fact, giving a means of finding this substance when all other processes have failed; and in *no case* of poisoning ought this viscus to be slighted at the *post-mortem* examination, the same care being used in removing it as in taking out the stomach and other viscera, for in the liver *the whole evidence* might exist.

Now to account for the sparing evidence of poison in the *fluid* contents. Pereira, in his "Materia Medica," vol. i., page 376, says, "Water, boiling, dissolves one twenty-fourth part of its weight of arsenious acid; on cooling it retains one-fortieth part of its weight; but by *cold digestion* it only retains one-thousandth or one five-hundreth of its weight. This is stated upon the authority of Mr. Taylor, in 'Guy's Hospital Reports,' vol. ii., p. 83." Could this have been the cause? I believe that it was. There is no evidence as to *how* the poison was taken, but it might have been swallowed in cold or tepid fluid. It was certainly suspended in fluid, since some granules of it still adhered to the stomach.

UNIQUE SPECIES OF HERNIA.

DEATH.—EXAMINATION.

MM. Carteron and Saussier, of Troyes, report the case of a woman, the mother of fifteen children, and who had, for about twelve years, had a crural hernia of the left side, very little perceptible externally, and which was usually without difficulty kept from protrusion by a truss. One day, during an effort at stool, the hernia suddenly pro-

truded. It was returned without difficulty by the patient; but after a few minutes a plucking sensation began to be felt in the left iliac fossa, which gradually extended to the umbilical region, increasing in intensity till violent colic was experienced. The woman was obliged to lie on her back, immoveable, and with the legs and thighs flexed; the temperature of the body was below the healthy average; pulse weak, and beating only from 45 to 50 per minute; the abdomen inflated; vomiting and various other symptoms were present, indicating the presence of a strangulated hernia. We may pass over the details of the treatment; it was unsuccessful and the patient died in less than two days.

On opening the body about two quarts of blood were found effused,—half into the cavity of the peritoneum, and half into that of the small intestine (*celle de l'intestin grêle!*). The stomach (duodenum) and upper part of the jejunum contained a small quantity of air; the lower fifth of the small intestine and the large intestine were entirely empty. "At the junction of the lower fifth with the rest of the small intestine, a mass of the ileum, eight inches in length, with a portion of mesentery, was found strangulated within a recent slit torn in the parietes of a herniary sac to be presently described. No perforation appeared in the intestine, nor was that portion of the mesentery engaged at all diseased.

The herniary sac, which extended from close to the uterus down to the crural arch, was attached at its extremity by firm bands to the thigh-joint (*racine de la cuisse*). It opened above by an oval orifice, capable of admitting the forefinger, a little more than an inch from the commencement at the uterus of the round ligament. It accompanied this ligament to near the internal inguinal ring, and then left it to pass under the crural arch, where it terminated in a cul-de-sac, capable of admitting a small egg. At a little distance from its orifice was a slit larger than the orifice itself, and opening from a supplementary sac (*laquelle termine sa infundibulum, un appendice veritable*), a subdivision, as it would seem, of the sac which may thus be considered double. This appendix, or supplementary sac, was about one inch and a half in length by a like breadth, and free in the abdomen. Its form was conical, its larger end being appended to the principal sac. The parietes of both sacs were formed by the peritoneal fold forming the broad ligament of the uterus. It was in passing from the principal into the smaller sac, and across this through the newly-formed slit, that the mass of small intestine and mesentery had become constricted. The mesentery had become involved at a point so near to its attachments to the vertebral column that by the attempt to straighten the body during life a considerable strain was exercised on the larger sac,—pulled thus in one direction by its attachment to the thigh-joint, and in another by those to the spinal column. The plucking sensations and pain had been accordingly felt by the patient especially on any attempt to straighten the thigh and leg of the affected side. The hernia had evidently become strangulated during the effort to pass the fæces above noticed, but though so short a period had elapsed between that occurrence and death, the parts involved had become so swollen that it was necessary to mutilate them to reduce the constriction.—*L'Experience.*

Sir Astley Cooper has recorded remarkable examples of both mesenteric and meso-colic hernia; but we are disposed to assent to the opinion of our French contemporaries that the above case, which they have named one of *hernia of the broad ligament*, is *sui generis*. It is obvious that in such a case surgical aid is of no avail.

WINE OF COLCHICUM IN FEVERS.

To the Editor of THE LANCET.

SIR,—I would beg to call the attention of the medical profession to the value of the vinum colchici in fevers generally.

I have used it for a considerable time in scarlatina, measles, small-pox, influenza, and every other form of fever, when there has been the slightest manifestation of an inflammatory action existing. In one case of confluent small-pox in an adult, who had been vaccinated, the most marked benefit was derived from its administration. I have also found it singularly useful in checking the evening exacerbations of febrile diseases, and in abbreviating the duration of the disease.

The only difficulty I have experienced has been its tendency to run off by the bowels, but after trying a variety of adjuncts I have proved that a minute quantity of the tinct. opii, modified by the addition of the same quantity of vinum ipecacuanhæ, has always retained the colchicum in the system, and, also, without at all increasing the febrile action by the presence of the opiate. Having for a lengthened period proved the advantage of the colchicum in such cases, I have wholly relinquished the saline method of treatment in fevers, and can with the greatest confidence recommend its employment to my medical brethren. I have the honour to be, Sir, yours obediently,

THOMAS EMBLING.

Brompton-row, May 18, 1843.

SPINAL MENINGITIS.

To the Editor.—Sir: I shall feel obliged by the insertion of the following well-marked case of a somewhat rare disease in THE LANCET. Your obedient servant,

E. C. TYTE, M.R.C.S.

Harrow, May 8, 1843.

M. M., a servant girl, aged 15, of a robust constitution, had been suffering for some days from severe pains across the loins, particularly over the spine and sacrum, which were supposed to be rheumatic. She had been frequently sitting and kneeling on the damp ground, weeding, and considered she had taken cold. She had never menstruated. She left her situation, came home, and took some diaphoretic medicine, which partially relieved her, and she returned to her occupation. In a few days, however, the symptoms returned with aggravated violence, and she came under my care. I found her suffering from excruciating pain across the lumbar spine and sacrum, passing round the hips to the fore-part of the abdomen, with deep-seated pain in both hip-joints, passing to the front and back parts of the thighs and legs, that on the right side being more severe; there was pain, also, along the crest of the ilium on the right side, extending to the front of the abdomen. She had a quick pulse, dry, furred tongue, and the usual symptoms of inflammatory fever. The body was bent forward, and inclined to the right side; the thighs approximated towards the abdomen, and any attempt to sit or stand upright gave the greatest pain. Upon examining her I found great tenderness over the lumbar spine and sacrum, with severe pain on percussion, together with a lateral curvature of the lower portion of the spinal column on the right side, producing obliquity of the pelvis and a lengthening of the right lower extremity.

The lumbar portion of the spinal column was clearly indicated as the seat of disease, by the local symptoms, and also by the fact that almost every part supplied by nerves proceeding from it was affected; and I came to the conclusion that the case was one of inflammation of the membranes of that portion of the spinal cord, the symptoms being those of irritation or spasm, such as this disease is known to produce, rather than the severer ones of paralysis, &c., resulting from inflammation of the cord itself.

The general appearance of the patient and the state of her pulse not indicating bleeding in any form, I contented myself with purging her pretty freely, and putting her under the influence of mercury, together with low diet, and the repeated application of blisters on each side of the lumbar spine, the recumbent position being enjoined. The pain was somewhat mitigated when the mouth became sore, and she began to sleep at night. The blisters were continued for six weeks, and the mercurial action kept up, when she became decidedly better. She lost all pain, could sit perfectly upright, the twisted state of the spinal column and the obliquity of the pelvis and lengthening of the right inferior extremity consequent thereupon, no longer existed, and her general health was very much improved. She complained of some uneasiness about the back of the right thigh, and upon examining I found the hamstrings tense and contracted, so that the knee was bent. Her appearance indicated debility; her pulse was small and feeble, and there was slight pain on percussion over the inferior lumbar vertebræ and sacrum. I considered the inflammatory action to have subsided, and that probably some effusion into the sheath of the cord might be keeping up this state of things. With a view of getting rid of the effusion, and at the same time of restoring the tone of the system, she took two grains of the iodide of iron three times daily, with meat diet and a pint of beer, a stimulating liniment being substituted for the blisters. Upon continuing this treatment for a month all the symptoms disappeared, and she is now a fine healthy girl.

The diagnosis of this case was not well marked, as in many of its features it resembled rheumatism, and indeed appears to have been brought on by it. It had also been mistaken for psoas abscess and considered a hopeless case. It has been remarked that diseases of the brain and spinal cord generally affect one side of the body more than the other. This was exemplified in the present case by the inclination of the lower part of the spinal column to the right side, produced by the spasmodic action of the muscles on the right side of the spine (the sacro-lumbalis, &c.), through the medium of the posterior lumbar nerves.

APHONIA CURED BY GALVANISM.

THEODORE MANDURIK, a Dalmatian, twenty-four years of age, of sanguine temperament and a robust constitution, and who had usually enjoyed good health, killed one of his countrymen in a quarrel, for which offence he was incarcerated in the prison at Scardona. Three days afterwards he was attacked by a violent fit of epilepsy, followed by entire loss of voice, to restore which external local and general bleedings, and antiphlogistic measures of all kinds were employed without effect.

In a few months he was removed to the central prison of Zara, where he was examined by the medical staff. The tongue was somewhat enlarged, and preternaturally reddened, though dry, and the blood-vessels around its base were much distended. The sense of taste was uninjured, but the movements of the tongue and of the larynx were performed with difficulty. Leeches were now applied to the sides of the tongue;

tartarised antimony, in both large and small doses, and drastic purgatives, were employed, and a tartar-emetic plaster was placed over the larynx; but all these means failed to restore a healthy action in the parts adjacent, and Mandurik was still compelled to keep his mouth partially open to maintain respiration, a function only performed by short and difficult inspirations. At length, about sixteen months after the attack, the voltaic pile was thought of, and a battery of fifty pair of plates was employed. The positive pole was placed over the cervical vertebræ, and the negative upon the parts affected. On the first day two hundred shocks were given, and on the second three hundred, but no perceptible effect followed. Two days were suffered to elapse, and a battery of seventy pair of plates was then used, with which about three hundred shocks were given. The patient was found acutely sensitive to the action of electricity, and a lapse of five days was permitted to intervene before its fourth application, which consisted of four hundred shocks with the last-named battery. Whether these had been administered too precipitately, or whether his system had become more excitable by galvanism, the patient after this last application became much agitated, and subsequently fainted for a short time. Next day he suffered intense headach, his face was flushed, eyes lustrous, pulse full and strong, from which state he was relieved by copious bleeding. But he now, for the first time, gave utterance to hoarse sounds. After six more days the battery of fifty pairs was again employed, and three hundred shocks were given. The same treatment was repeated every two or three days, and then, at similar intervals, four hundred shocks were given with the seventy-pair battery. The voice, meanwhile, and the motive powers of the tongue and larynx, gradually returned to their normal condition, and after the twelfth application the patient had completely recovered. The deduction drawn by the surgeon who has reported the case is, that no nervous affection whatever should be regarded as incurable till electricity in some form has been tried and found to fail.—*Giornale de Progressi della Patologia e della Terapeutica*, Venice, Jan. 1843.

ASPHYXIA.

M. Le Roy d'Etiolles asserts, in opposition to Bichat, that the effects of asphyxia, from any cause, are not experienced by the brain alone; but that the heart, for instance, feels, in a primary degree, the deleterious influence of venous blood, under the presence of which the contractions of the left ventricle speedily become enfeebled.

CASE OF INTUS-SUSCEPTION IN A CHILD.

At the last meeting of the Westminster Medical Society Dr. Chowne related the following case:—

A child, three months old, a patient of Mr. Weatherfield's, of Henrietta-sreet, Covent-garden, by whom it was vaccinated at about twelve o'clock in the day; at that time it was in good health; during the operation it screamed and struggled a great deal, apparently from the pain and from restraint of position; after this it slept about two hours. It was noticed that the child upon awaking did not appear well; in the evening of the same day it was evidently suffering, and at the request of the mother Mr. Weatherfield visited it. The abdomen was found rather tumid, and there was a discharge of thin fluid from the bowels, mixed with a little blood, but no proper motion. A powder, consisting of rhubarb and grey powder, with a carminative mixture, was prescribed; the next morning the child was much worse; the abdomen was tumid and tender. On being pressed there was a greater discharge of thin fluid mixed with blood, still no motion had passed; there was great anxiety of countenance, and vomiting of bilious matter. At this time a hard lump. about the size of a hen's egg, could be felt in the right iliac fossa; an enema was administered, but came away without producing any good effect. During this day, the second, two grains of calomel were given, with other aperients, but without the slightest relief; in the evening Mr. Weatherfield introduced, though with some difficulty, air into the bowels by means of an enema-syringe, but without succeeding in making it pass beyond the obstruction; in the course of the evening the child died, about thirty-three hours after the commencement of the unfavourable symptoms. The quantity of blood that passed away from the rectum appeared to be about four ounces, a quantity sufficient to be regarded as adding considerably to the exhaustion in a child so young.

An examination of the body was made on the following morning by Mr. J. Moore, resident surgeon in Charing-cross Hospital.

The exterior of the body presented nothing unusual, except that the situation of the tumour which had been previously felt, was rendered obvious by the elevation of the parietes at that part. This fulness presented so strong an indication that the seat of the obstruction was there, that it was made the first object of investigation, and the abdomen was accordingly opened. The peritoneum, and the intestines generally, appeared to be healthy; there was neither deposit of coagulable lymph nor of fluid. Upon turning aside the portions of intestine which lay over the tumour, the seat of disease was arrived at, and its nature ascertained. A ligature

was put upon the colon, another upon the ileum, and the intermediate portion of intestine removed. Upon examination it was found that the lower portion of the ascending colon, where it is continuous with the caput coli, was turned inwards upon itself, and that this inflected portion inclosed the cœcum, which was again in part everted upon itself, or turned (so to speak) "inside out;" the appendix vermiformis and about an inch and a half of the termination of the ileum, were included in the colon. The coats of the cœcum were exceedingly congested; the mucous membrane very soft and swollen; the thickness of the gut much increased, and the cellular tissue between its coats infiltrated with serum. There was within the colon a small quantity of thin brownish-yellow fluid.

Dr. Chowne mentioned this case, not as very rare, but as presenting peculiarities which are more characteristic of intussusceptio than of obstruction within the bowels from any other cause, although it could not be considered as a common occurrence that a lump should be felt in the abdomen; and although absence of such lump would not negative the supposition that intus-susception existed, yet the presence of a hard substance very much strengthened the suspicion, and a gradual but pretty constant escape of bloody fluid from the rectum constituted a strong indication of that condition.

After a short address from the president, Mr. H. J. Johnson, the society adjourned until October next.

PROFESSOR GROVES'S NEW BATTERY,

IN REFERENCE TO THE

ELECTRICITY OF ANIMALS.

To the Editor of THE LANCET.

SIR,—On reading over Professor Groves's experiments with his new gaseous voltaic battery,* a point has struck me, with regard to the source of the electricity in animal bodies,—which no doubt exists whether it be identical with the nervous influence or not. In the voltaic pile it is certainly the oxygenation *of a metal* that greatly increases, if not actually causes, the evolution of the electric influence; but the oxygenation of *the blood* is a chemical action, so entirely different from this, that it always seemed very doubtful whether we could logically infer that the combination of oxygen in this case likewise produced electricity. Accordingly, Dr. Prout, many years ago, only, I believe, broached the hypothesis as a probability. Now, Professor Groves's discovery seems to have brought us one step nearer certainty on this point, for it shows that when oxygen *combines with a gas*, and forms water, electricity is likewise produced; for Mr. Groves has produced the shock, spark, chemical decompositions, &c., by fifty pairs of the platinum oxygen-hydrogen battery—has decomposed the iodide of potassium, indeed, by four pairs only. Organic chemists, therefore, have only to show that water is actually formed by the union of the oxygen absorbed *with hydrogen in any part of the circulating system*, and they have at once *proved* that a good deal of electricity must be disengaged. But further, this discovery of Mr. Groves's has enabled us also to affirm, as certainly as can be done without experiment, that electricity is also evolved when *oxygen, by uniting with carbonic oxide, forms carbonic acid;* for platinum is well known also to have the power of causing the formation of carbonic acid as well as water, when carbonic oxide is let up with a mixture of oxygen and hydrogen; *indeed, to form this before it forms water.* (Dr. Henry, Phil. Trans., 1824.) Now, although Mr. Groves does not appear to have tried carbonic oxide in his battery, there can be little doubt (as platinum makes this combine with oxygen before it makes hydrogen) that much electricity is evolved by the formation of carbonic acid *at low temperatures;* for, I believe, many years ago Laplace showed that it was by the combustion of carbon. It is also remarkable, and in favour of this general view of the production of electricity by respiration, that olefiant gas is also acted upon, though not *until after* the hydrogen and oxygen have united, and carburetted hydrogen not at all. (Henry, Phil. Trans.) Now, it is far more probable that carbonic oxide is formed in the course of the circulation than either of the above gases.

It may be objected to the above that we have no platinum in the animal system; but the platinum, properly considered, seems only to be regarded as, in some way or another (for Mr. Groves asks, Would not gold and silver act?), as a conductor, making the electricity evolved *evident.* "The gases diminish, and the hydrogen much more rapidly than the oxygen;" *so here is the oxidation of the hydrogen, and this only.* I now have only to add that, nevertheless, M. Schœnbein "*thinks* (Phil. Mag., March, 1843,) that oxygen does not contribute to the current." He admits, however, that he has not yet tried the experiment of having *all* the tubes filled with hydrogen; but says that hydrogen and water produce a current when platinum completes the circuit. However, Mr. Groves tried common air in *all* the tubes, carbonic acid and nitrogen, and oxygen and nitrogen in alternate tubes, with no effect, and his theory seems to be that the *union* of oxygen and hydrogen is the cause of the current. I am, Sir, your most obedient servant,

H. PRATER.

May, 1843.

* Phil. Magazine, Dec. 1842.

RARE CASE OF

FILAMENTOUS OR ENTOZOON WORMS.

To the Editor of THE LANCET.

SIR,—In your Number for the 13th of May a correspondent, who signs himself a "CONSTANT SUBSCRIBER," solicits a reply to the following question:—"Is there a disease of the skin where living animals, or insects, are turned out?"

I would refer the inquirer to almost any of the works of generally accepted authorities on diseases of the skin, wherein he will find many of them that take their names from the very circumstance of the presence of organised living animalculæ, generated from larvæ deposited in the true skin and its tissues. The æstrus, for instance, so common in cattle and sheep, and which are recorded to inhabit the human body, by persons to whom every credence is due. The gordius, or guinea-worm, is another instance. Some mostly exist on the epidermis, but, nevertheless, penetrate its tissues and substance, as the pulex penetrans, the jigger of hot climates, the acarus scabiei, &c. &c. I will only allude to the numerous parasitic animalculæ so commonly attendant on various diseases of the surface, where the integrity of the skin is broken, where the health is broken down, and often from want of mere cleanliness.

I would now briefly relate a case, amply confirming that living animals do infest the human body; it is the only case of the kind I have ever seen, but it will, perhaps, be sufficient to satisfy your correspondent.

Nearly five years since a lady came to take the sulphur-fumigating baths in Great Marlborough-street. Having taken a sulphur fumigation, in the evening the female bath-attendant told me she had that day had a very queer case; that the lady thus spoken of, on coming out of the bath, was covered with hundreds of small worms, some nearly an inch long, which had continually kept jumping out of the skin whilst the lady was dressing. Although, from the respectability of the attendant, I ought to have placed more confidence in what she said, I hastily told her not to tell me such nonsense. The next day the lady had another fumigating bath, and a similar report was made to me, but more exaggerated in detail than the first report, and to which I again paid little attention. The lady had a third fumigation,—the same effect was produced, and again reported to me. After taking the fourth fumigation, the lady, hearing of my unbelief, requested my attendance in the dressing-room, to witness the fact. I then saw hundreds upon hundreds of these worms escaping from the skin of the legs, arms, and body; the carpet where she had stood whilst dressing was covered with them. They kept jerking themselves from the skin—a sort of jumping—and they had the power of ejecting themselves to the distance of twelve to twenty inches, as nearly as I could guess. With a towel I wiped some of the parts myself, where I saw these worms projecting from the skin; some of them extended more than half an inch from the surface, and these, when approached by the towel or finger, ejected themselves out with considerable force; others, not more out than about a quarter of an inch, were broken off by the towel, wiping, and those that were only out about the eighth of an inch would retract themselves for a few seconds, and then again show themselves ready for and in progress of escape. On putting the towel on the table I was astonished to see it covered with these animals, that dropped from the folds of the napkin. They were, for the most part, about an inch in length, but there were others of various shorter lengths. They were pink in colour, and transparent, with dark brown heads, though the larger ones had the heads quite black; these appearances were amply distinct without a glass. In thickness they were about the size of a strong hair or bristle. During the few succeeding days I had an opportunity of showing some hundreds that I collected to several medical friends that called on me. I have some dozens by me still, of course much shrivelled, and looking like short black hairs. I shall be happy to show them to your correspondent, or any other medical gentleman curious on the subject.

The lady had but few fumigations, and I suppose got well. I never knew who she was, or who sent her to take the fumigations. The nature of the case, perhaps, with her governed this cautious reserve. The only time I saw the patient was the time I have named. She said she had been teazed with these worms for more than two years. They were continually falling on the carpets, occupying the coverlid and bed linen. She had consulted several of the leading practitioners of the west end, some of whom she named, but had received no relief from what had been prescribed for her, and she attributed her ailment from having fallen asleep in the summer, when lying on the ground, near some stagnant water, and on waking found her mouth almost filled with something very like small worms. A short time afterwards she found her body infested with them, and they had until the time I saw her been continually on the increase.

I am aware that such cases as here related are not familiar to the profession, and have nothing more to offer as to the cause or nature of these entozoon worms, but would refer the inquirer to the appendix of Rayer's valuable "Theoretical and Practical Treatise on Diseases of the Skin," but perhaps more particularly to those naturalists and entomologists who have made this subject a principle

of inquiry, as Blainville, Etmuller, Bassignot, &c.; my object in this paper was merely to state the few facts I am in possession of, and which I myself witnessed.

JONATHAN GREEN, M.D.

Great Marlborough-street,
May 19, 1843.

*** We take this opportunity of saying that the note of the correspondent referred to at the commencement of this letter found its way into print accidentally, the name which was contained in the question and reply not being intended by the editor to appear publicly.

NEW MODE OF ADMINISTERING MERCURY,

AND THEORY OF ITS CHANGES IN THE SYSTEM.

M. MIALHE is consider to have established that all the preparations of mercury employed medicinally act by being changed in the animal economy into the bichloride, to the action of which preparation their therapeutic and toxicological effects are owing. This chemical change is produced by the alkaline chlorides present in the humours of the animal frame. The proportion of bichloride thus formed is partly in correspondence with the quantity of the alkaline chlorides present; but partly also dependent on the kind of mercurial preparation taken,—the deuto-salts being at once converted into corrosive sublimate, while the proto-salts pass first into the state of proto-chloride, and only attain their final composition and efficiency by a secondary change. Finally, the bichloride of mercury forms, in conjunction with the alkaline chlorides and the albuminous materials of the blood, a chemical combination susceptible of traversing the whole vascular system without appreciable alteration.

Acting upon these data, M. Mialhe, in order to place the animal system as early and as certainly as possible under the influence of mercury, proposes the employment of what he calls a normal solution of mercury (*liqueur mercurielle normale*), and which is composed as follows:—

℞ *Bichloride of mercury*, four and a half grains;
Chloride of sodium, fifteen grains;
Muriate of ammonia, fifteen grains;
The white of an egg;
Distilled water, a pint.

Beat the white of egg with the distilled water; filter; dissolve the three ingredients in the fluid, and filter anew.

In an ounce of the above solution there is consequently rather more than a quarter of a grain of bichloride of mercury.—*Journ. des Conn. Med. Prat.*

ON THE MECHANISM OF ABSORPTION.

By GEORGE ROBINSON, Esq., M.R.C.S.L., Fellow of the Royal Medico-Chirurgical Society, London.

IN a memoir which was read before the Royal Medico-Chirurgical Society in February last, I attempted to prove, by a series of experiments, that a partial or complete obstruction to the passage of the blood through the smaller vessels of the body will cause the escape of its albuminous portion through their coats. In the present communication I wish to direct attention to the influence which the opposite condition of the circulation exercises in promoting the absorption of any fluids that may be placed in contact with the external surface of the vessels, or only separated from them by one or more membranes.

If we examine the facts brought forward to illustrate the process of absorption, we shall find that the following is, perhaps, the only general conclusion that can be safely drawn from them, viz.:—

That no substance can exert any influence on the whole system unless the circulation of the blood, through the vessels of the part to which it may be applied, is performed with a certain degree of activity.

That the circulation of the blood must precede and accompany the act of general absorption is, among many other instances, proved by the following experiments:—

1. Magendie divided all the parts of the thigh of an animal but the femoral vessels, and then inserted poison under the integuments of the limb. While the vein was compressed no symptoms appeared, but they were immediately produced when the return of the blood was unimpeded.

2. Emmert tied the abdominal aorta, and introduced poison into a wound in the foot, but at the end of seventy hours no effects had appeared. The vessel being then liberated the poison (prussic acid) acted within half an hour. I have repeated the experiment with precisely similar results. The aorta being compressed, a few drops of a strong solution of hydrosulphate of ammonia were introduced beneath the integuments of the thigh. At the end of seven minutes no symptoms of poisoning had appeared, but on then releasing the vessel the operation of the poison was immediately evident, and the animal was dead in less than a minute. A number of similar facts might be adduced in support of this point, but it will be sufficient to refer for them to any modern work on physiology.

This principle being established, we may, in the next place, proceed to inquire how the action of the circulation, in favouring absorption, is to be explained.

According to the laws regulating the

transmission of *stagnant* fluids through membranes, exosmosis and endosmosis should be continually occurring through the coats of the vessels of the living body; and there undoubtedly analogous processes do take place. But it appears to me highly important that we should ascertain how far those laws are modified *when one or both fluids are in a state of motion*. However, Magendie, and all succeeding physiologists who have adopted his opinion, suppose that the external fluid permeating the membranous coats makes its way to the internal surface of the blood-vessel, and is *then* swept on by the current of the blood.

According to this hypothesis the chief part of the process of absorption is made to reside in some peculiar power possessed by the membranous coats of the vessels,—the circulating fluid itself being quite a secondary agent, though, as above stated, its motion is necessary for the action of any poison on the system at large.

To prove the truth of his opinion Magendie performed an experiment. He passed a stream of water through a portion of carotid artery and jugular vein, having previously applied strychnine to the exterior of the vessels. In a few minutes the water discharged had acquired the bitter taste of the poison.

Now this experiment, considered by itself, throws little light on the mechanism of absorption; it merely proves that while a stream traverses a membranous tube any soluble matters placed on the exterior of that tube will be absorbed; but as to the relative influence of the stream and the membrane in accomplishing that end, we are left altogether in the dark. He referred the whole of the transmitting power to the membrane, but the same experiment will, I think, prove equally well the influence of the stream in increasing that power.

It has long been known that a fluid, while traversing any porous vessel, will draw in a considerable quantity of air, which is liberated when the fluid is again at rest.

I now proceed to quote an experiment from Sir John Leslie's "Elements of Natural Philosophy," vol. i., p. 364:—

If a cylinder, "one inch in diameter, and three inches long, be fitted into an orifice at the bottom of a cistern, and on its upper side, at the distance of half an inch from its origin, a narrow arched glass tube be inserted, the long end of which is carried down to a basin of water, three feet below the insertion of the other end,—when a stream traverses the cylinder with a velocity of nine feet per second, it will raise the water up the glass tube to the height of two feet, and if the tube be shortened within that limit, the basin will in a short time be emptied of the stagnant fluid." I have now several times repeated this experiment, and with a rapid stream the results were always in accordance with the above statement. Having filled a wine-glass with coloured fluid, and having connected its contents (by means of a bent tube, twelve inches long) with the interior of a pipe half an inch in diameter, I found that the glass was drained of the stagnant fluid in less than two minutes after the stream was turned through the pipe.

I may refer to the same page for a proof of the statement that this imbibing power of the stream is proportioned to its velocity.

I will mention one other proof. While a cistern is full the rate of discharge and consequent velocity of the stream through the orifice at its bottom will, of course, be greatest. Now, if the apparatus above described be fitted into a pipe connected with this orifice, it will be found that the stagnant fluid will rapidly rise to a certain height in the glass tube; but as the depth of the column of water in the cistern diminishes the fluid in the tube will oscillate and fall till the stream becomes so tardy as not to exert any marked influence on the stagnant fluid.

Having now proved the existence of this power, and also that it is proportioned to the velocity of the stream, the first step towards the application of this principle to the explanation of the process of absorption in the living body, is to show that the same force, whatever it may, be will act through one or more membranes.

Accordingly, the short end of the glass tube being covered with membrane and its long end immersed in a coloured fluid, I found that within five minutes after the stream had commenced to flow, the whole of the air present in the tube (which was twelve inches long and its bore one-fourteenth of an inch wide) had been absorbed and its place supplied by the coloured fluid. A continued slow ascent of the latter was still proceeding when I was compelled to stop the experiment. I afterwards repeated it, covering the short end of the tube with two membranes instead of one, but using a tube several inches shorter than in the last case, and found that there was a very slow, but steady and constant, flow of the fluid towards the stream; the fluid which thus traversed the tube being opposed in its flow by gravity, and being itself of greater specific gravity than that of the water constituting the stream.

By these direct experiments, and from a careful consideration of the chain of indirect evidence furnished by a number of morbid and healthy actions that are known to occur in the bodies of animals, I am, I think, justified, even in the present incipient stage of the inquiry, in making the following statement.

That as it has been shown that the effusion of albuminous matters through the coats of the vessels of the living body is produced by and modified in its nature ac-

cording to the degree of the compression of the blood contained within them, this action being independent of and even in opposition to the ordinary laws of exosmosis as deduced from experiments on *stagnant* fluids. So I believe the chief part of the process of absorption in animals to arise from and depend on a force existing within the blood-vessels, that force being generated by and proportioned to the velocity of the moving mass by which, in a healthy state, they are incessantly traversed.

That something like a suction-power acts in promoting or causing absorption is undoubtedly a very old opinion, and the truth of this remark is evident in the literal signification of the very word. But so far as my present knowledge extends, all previous theories have referred that power either to the enlargement of the chest in inspiration or to the dilatation of the ventricles.

Now, while readily admitting the influence of these two actions in facilitating the return of venous blood, and thereby indirectly favouring absorption, I cannot think that they exercise any immediate effect in promoting the entrance of extraneous matters through the coats of the smaller vessels.

But if, on the other hand, we suppose this suction-power, whatever its precise nature may be, to be caused by and reside in the stream of blood, to be greatest wherever the current of the latter is most rapid, to be increased in its activity by whatever accelerates the velocity, and diminished by whatever induces a retardation, of the circulation through the part, then we shall have a principle of action, at once simple and efficacious, in perfect accord with the established laws of matter, and by the operation of which many morbid and healthy phenomena of the human body can be satisfactorily explained and readily understood.

In the present stage of the inquiry I shall not attempt to make many applications of this principle, but content myself with briefly alluding to some facts which seem to corroborate the doctrine herein sought to be established.

Magendie proved that a state of plethora retarded, and one of depletion promoted, absorption. These two conditions of the system are evidently attended by two opposite degrees of activity of the circulation. For when the mass of blood is much augmented the obstacles which naturally oppose its free passage through the smaller vessels are materially increased, at the same time that the action of the heart is slower and more laboured than in health. Consequently, the proportion then existing between the moving power and the mass to be moved is unfavourable to a rapid circulation; and we may thus explain the slower action of poisons after he had injected a quantity of water into the veins. Whereas, after bleeding, or any other large evacuation, the whole of the arteries contract as the quantity of blood in the system diminishes, so as to form a series of narrower tubes, and the heart being relieved of a part of its load, after the first shock is over, increases the rapidity of its contractions so much that a more rapid circulation of the blood must necessarily result.

There are two parts of the animal economy where the process of absorption is actively carried on, and where some beautiful modifications of this principle are exemplified. I have said that compression of the blood within its vessels causes effusion; it should follow, therefore, that the compression of an external fluid against membranous tubes, while traversed by a rapid stream, will promote absorption; and such is known to be the case. The influence of pressure in causing the absorption of morbid effusions in the human body is well known, and its application is seen in daily practice. In the intestines, through which the bodies of animals obtain all their nourishment, some provision for increasing the absorbing power of the blood-vessels was required, and is accordingly there found. Their peristaltic action keeps up a steady continued pressure of the chyme against the vessels, thereby regulating, to a certain extent, the quantity of fluid effused from the latter, at the same time that their calibre is diminished and the velocity of the blood traversing them consequently increased. It has been an interesting problem in physiology to explain how the fœtus is nourished by the maternal blood without any direct vascular communication existing. On viewing the maternal vessels it is evident, from their increased diameter, great length, and tortuous arrangement, that the circulation through them must be much retarded; while, on the other hand, the shorter circuit of the fœtal blood and the more rapid contractions of the fœtal heart, tend to prove that the velocity of the fœtal blood during its passage through the placental vessels is much greater than that of the maternal circulation in the same part.

Since, therefore, it has been shown that the absorbing power of a stream is proportioned to its velocity, it should follow that when two currents of blood moving with different degrees of velocity are in juxtaposition with each other, being separated merely by two thin membranes, the more rapid stream will not only draw in any fluid that may have been already effused by the other, but may even directly increase the rapidity of that exudation.

In whatever part of the body the course of the vessels is tortuous, or their coats are relaxed, a retardation of, and consequent obstruction to, the circulation of the blood results, effusion takes place, and absorption is performed very slowly if at all; and with opposite conditions of course we have opposite effects. We can thus understand

why the surface of old ulcers should absorb more freely than that of recent ones; why a ligature tied round the limb impedes the action of poisons; and why the same beneficial result attends the application of a cupping-glass over a poisoned wound. In short, throughout the whole body the two antagonistic processes of effusion and absorption are constantly in operation, the preponderance of the one or the other in any particular part being regulated by the facilities or impediments afforded to the passage of the blood through the smaller vessels of that part. And I believe that absorption takes place chiefly in the smaller vessels, being facilitated there by the tenuity of their coats and the rapidity of the stream traversing them. As before stated, I consider these two processes to be in their nature and causes essentially different from those usually understood by the terms endosmosis and exosmosis, and to be in a great measure independent of the laws regulating the transmission of *stagnant and uncompressed fluids* through membranes.

Hunter-street, Brunswick-square,
May 19, 1843.

P.S. Of course these remarks are intended to apply to the general processes only; for there is undoubtedly something more than a mere physical law in operation in all those parts where secretion is going on; and a certain selective influence is probably also exercised in regulating the admission of external matters within the circulation.

FURTHER REMARKS BY

DR. HASTINGS ON HIS TREATMENT OF TUBERCULOUS CONSUMPTION WITH NAPHTHA.

To the Editor of THE LANCET.

SIR,—It was to be expected that in offering to the profession a remedy for such an hitherto incurable disease as phthisis, that it would be received, at best, with much distrust, and by many with total disregard. More could hardly be hoped for, when we reflect on the frequency and fatal nature of the disease, the consequent attention bestowed on it by many of the ablest men of all ages, and the numberless agents which have been uselessly extolled and as uselessly employed to check its ravages, I only hoped that enough had been stated in the "remarks on tuberculous consumption, and its treatment with naphtha, with experiments and cases," in THE LANCET of April 29th (p. 149), to obtain a fair trial for this new remedial agent. I may add that nothing has yet occurred either in the cases reported, three of which I have seen during the past week, or in those subsequently treated, to shake my faith in its efficacy.

My reason for addressing you again, at this moment, is for the purpose of replying to your courteous correspondent, "S. C. D.," who very fairly observes, and in which I fully concur, that "it appears to me that to decide on the relative superiority of naphtha, even as an ameliorating agent, it would be necessary to select a certain number of cases, of average tuberculous malignity, part treated with naphtha, and part by other remedies, and let the result determine."

"S. C. D." appears to consider my cases to be neither more or less than cases of winter-cough, and only on the ground that my patients had cough in the previous winter. This point will be readily cleared up by inquiring what are the symptoms which indicate the presence of winter-cough; and what, tubercles in the lungs: Winter-cough, or, in other words, chronic bronchitis, occupies generally the lower lobes of both lungs, over the surface of which slight dulness on percussion is occasionally elicited, and upon auscultating the space, sibilant, sonorous, or submucous râles are heard; the secretion has more or less a frothy character, mixed with a denser semi-transparent mucus; the motions and form of the chest are rarely altered, and emaciation is by no means a constant attendant. This affection is seldom met with in persons who have not reached the middle or advanced periods of life. It will be well to observe that two of my patients were under twenty years of age, three under thirty, and the oldest no more than thirty-four. Their prominent symptoms were general emaciation, dulness on percussion over the superior portion of the chest, where the breath-murmur was weak, harsh, and inaudible, with augmented heart sounds, particularly on the right side, &c. Such was the disease that I treated with naphtha; as to the treatment of chronic bronchitis with the same remedy I have no experience. I have carefully abstained, hitherto, from treating cases of an equivocal nature with naphtha, in order that I shall neither delude myself nor others into the belief that tuberculous consumption had been treated, when it was really some other affection, of a less fatal nature, and for which we have already abundant remedies, and in this way obtain, perhaps, a therapeutical character for naphtha it would have justly no claim to.

Again, "S. C. D." says, "to obtain for naphtha the character of a specific, it ought to be tried not upon winter-coughs, but upon cases yielding in *summer* all the stethoscopic signs of tubercle, &c." Might I ask "S. C. D." if he considers a disease that affords all the symptoms of phthisis in the *summer*, would fail to do less in the *winter*? For my own part, I must repeat that winter is

the most unfavourable season of the year for treating tuberculous consumption; indeed, this is an almost universal opinion, and consequently requires no additional testimony from me.

I must again call the attention of those who intend making trial of naphtha in this disease, to be extremely particular in the kind employed, as several accounts have reached me where its use has been entirely abandoned in consequence of sickness, palpitation of the heart, &c., it occasioned. If the naphtha mixes with water without producing a cloud, and is as transparent and colourless as distilled water, and of the specific gravity .824, it will, according to my experience, produce no such untoward effects; but, on the contrary, will augment the appetite and exalt the system.

I shall shortly forward to you another report of cases, including one, at least, successfully treated where softening and caverns existed in the lungs.

Since the foregoing was written, I have read with much interest Mr. Atkinson's letter in to-day's LANCET on this subject. His principal objection to my cases is founded on incorrect data, hence much of its force is lost. He says "the whole of the cases in Dr. Hastings' report were in the month of March and the first week in April," and on this he grounds his argument. Instead of which, the treatment of three of the cases, out of the six reported, commenced in December, and another in January.

With regard to creosote, I thought it had been laid aside long since as a medicine of no value in phthisis. Perhaps no one gave it so extensive a trial in this disease as Dr. Elliotson, both through the medium of inhalation and the stomach, and I know that he only discontinued its use from the conviction that it was utterly useless in that affection. I might cite other eminent authorities, holding the same opinion, were it necessary, but shall conclude with a quotation from Gully's Translation of a Formulary, &c., by Magendie, 1836, page 207:—"Creosote has been employed by the French physicians in pulmonary phthisis, but from all that I have read on the subject the alleged successful cases are strained, and should not be recorded as such." I am, Sir, your obedient servant,

JOHN HASTINGS, M.D.

Albemarle-street, May 20, 1843.

A COOL RECEPTION.—Dr. Scholler, of Berlin, proposes that when new-born infants appear dead from suffocation little time should be lost before they are treated with a hasty plunge in cold water. This, he says, will often restore them speedily when other means have failed, though he cites a case in which three hours were spent in successive immersions before the infant respired!

INSANITY.

THE ADVANTAGES AND RISKS OF MANUAL FREEDOM.

To the Editor of THE LANCET.

SIR,—The question of restraint to the limbs of the insane has, by most of your correspondents, been advanced, not in respect of its merits or its demerits as a remedial agent, but as a means of preventing bodily injury. Having in my former communications (pp. 12, 144) slightly touched on the consideration of it as a curative application, I now propose to confine the few remarks that I am about to make to the examination of the bodily injuries which are generally sustained by patients who are suffering from insanity. In doing this, I shall endeavour to draw some comparison between the amount and the nature of the injuries that are sustained under both plans of treatment.

At the outset of an inquiry into the comparative proportion of wounds, bruises, and sores, following the adoption of the one or the other mode of managing the insane, a very important distinction presents itself; namely, where the patient is fastened, the injuries arising are sustained chiefly by the patient himself, and when not fastened they are presumed to fall on the persons of others. This is a manifest and important distinction, and one which it is expedient, in the consideration of this subject, that we should bear constantly in mind.

A further distinction is apparent in the nature of the injuries sustained. Patients under restraint are liable to suffer from a class of injuries consisting chiefly of ulcerations and mortifications of parts, produced by the continued pressure of the restraining agent on the limbs, or by the weight of the body kept constantly in one position on the bed. When left unrestrained the injuries consist chiefly of contusions or wounds inflicted by one patient on another, or by a patient upon his attendant.

Nice points like the one now under consideration cannot be determined with the closest exactness without the most extensive experience, and without a series of well-recorded observations. I conceive, however, that at the present time we possess that amount of knowledge which enables us to determine the question whether, under the "restraint or the non-restraint systems," the greatest amount of bodily suffering is experienced.

It is at once evident that by removing from the patient instruments which cause pressure on the limbs, and keep his body in one and the same recumbent position, the class of injuries before alluded to, namely, ulcerations and mortifications, is entirely done away with; and let me remark, it is no slight mitigation of suffering to abolish the

torture arising from the extensive ulcerations and mortifications of the limbs and backs of our patients. Formerly, such destruction of the surface of the body was commonly considered as the necessary and inevitable accompaniment of certain disordered conditions of the mind, and the practitioner doubtless conceived he had accomplished and carried out all that lay in his power when he had devised less galling shackles, and ordered soothing appliances to the sore and irritated parts.

Having disposed of this class of injuries, we have now to examine whether those inflicted by patients on each other, or by patients on their nurses, are increased when the treatment is undertaken without having recourse to mechanical coercion. So far as I am warranted by experience to draw a conclusion on this point, I can truly affirm that freedom of action has not been followed by more frequent or more severe injuries than I have been in the habit of witnessing under the old method of treatment. On the contrary, I confidently believe that there has been a decided diminution, both in severity and frequency, and I am firmly of opinion that as the nurses and attendants become better acquainted with the principles on which we act, a still greater diminution in the number of accidents will then take place.

In order that the question might be put in as simple a form as possible, I have in the foregoing remarks avoided making allusion to several considerations which ought also to be taken into account in drawing an estimate of the amount of bodily suffering inflicted in the treatment of insanity.

On the one hand, I have kept out of view the permanently contracted condition of the joints which has been known to result from the fixed position of the limbs; also the injuries arising to the patient when the body and limbs are only partially secured, when he is liable, by frequent chafing and striking of the limbs or head, to inflict a considerable amount of injury upon himself; also the instances of severe injury to others by patients who have escaped from the restraining agents, and who have used their irons as instruments of aggression on their keepers. I have avoided, also, taking into account the irritating effects of urine on the nates of those secured to their beds.

On the other hand, I have not alluded to the injury that a patient unrestrained may sustain by striking his person against the floor or walls of his apartment, or the increased facility he may possess for the commission of suicide. The consideration of the two latter points would of themselves have opened the question to an extent much beyond the limits of this letter, as it would have been necessary to go into details descriptive of the means devised to prevent such occurrences.

It has been said that the practice of treating insanity without having recourse to restraining agents is unsafe; that is to say, I presume, that it is attended by an increased liability to bodily injury. If the calculations which I make be true, we may assert that the practice of treating insanity by coercive means is unsafe, for it is the cause of a greater amount of bodily injury than arises when the patients are treated without recourse to such means. Thus, do observation and experience show that the very grounds on which the chief argument has been founded in support of mechanical coercion are utterly false, and are, in fact, when thoroughly scrutinised, found to favour the opposite practice.

To draw a general conclusion, we may, I think, take as a fair calculation that, by undertaking the treatment of insane patients without fastening them, we remove from them the whole amount of suffering that is attendant on ulcerations and mortifications of the surface of the body, and also diminish the amount of injury arising from other and accidental causes. I remain, Sir, your very obedient servant,

G. J.

May 19, 1843.

Statistics of Fractures.—It appears, from the report of MM. Manoury and Thore, that in 1841 there were treated in the Hôtel, Dieu 109 cases of fractures, of which 40 occurred in January, May, and October. Out of the entire number, also, 8 had fractures of more than one bone, viz., 5 had 2, 2 had 3, and 1 had 4 fractures,—seven of these subjects having received their injuries by falls from high places; the multiplicity of fractures, however, in the same subject not having been found to retard the cure. With respect to the proportion of members fractured, it appears that out of 121 cases, 36 were fractures of the superior members; 54 of the inferior; 12 of the skull; and 19 of the spine and trunk; and further, comparing the fractures according to the sides of the body in which they occurred, 64 were found on the left, and only 39 on the right side,—a very different statement from that of Dupuytren, who assigns 7-10ths of these fractures to the right, and only 2-10ths to the left side. The small number of cases, however, from which the former conclusion is drawn makes it obviously of little value. MM. Manoury and Thore further state that of the fractures of the limbs (most of which are caused by direct violence), 20 were of the right and 19 of the left arm; 35 of the left and 19 of the right leg. The fractures of the clavicle were seven; all on the left side. The eight fractures of the skull were all fatal.—*L'Experience.*

REMARKS ON

INFANTILE PARAPLEGIA, WITH CASES.

By HENRY M'CORMAC, M.D.

THERE is a disease of very considerable frequency—I mean *infantile paralysis*—to which, I think, so much attention has not been given as its importance merits; and, while speaking of paralysis, I advert particularly to that form of it which bears the aspect of paraplegia.

Infantile paraplegia has come at least a dozen times under my observation, but I have no exact means of knowing how often it presents itself to others; if, however, it prove to them of the same recurrence as to myself, the disease, in the aggregate, must be one of no small prevalence.

Sometimes one lower extremity is affected, oftener both. The child may be wholly deprived of the power of locomotion and station, or this may be only partially impaired. Somemties the disease comes on rapidly, while at other times the impairment is very gradual. It may ensue, so far as I have seen it, during lactation, or not till the second, third, or fourth year of the child's age.

The aspect of an infant labouring under this disease is very peculiar. If able to walk at all, he lifts and plants his feet very slowly; while at each step he raises his shoulder with an effort as if he were bearing a heavy weight. If a single limb be affected, this mode of progression is manifested on one side only; but if the child be wholly debarred from locomotion, he lies prone or supine on the nurse's knees, or on his little couch, the countenance at the same time displaying the usual anxious expression which attends impaired health in infancy.

The causes of this malady are very obscure. From the progress of one case, complete paraplegia, wasting of the limbs, incontinence of urine and fæces, in a child of ten months, I should say the spinal marrow were diseased, perhaps tuberculous. In other instauces, I should say the spinal marrow had ceased below a certain point to give out its nervous influence, but not otherwise to labour under serious structural lesion. In cases of this last description I think it extremely probable that the medulla suffered from the effects of concussion, resulting from falls and blows, often kept secret, to which children, owing to the careless hands into which they are often intrusted, are unhappily liable. When paralysis is confined to one limb I should think it likely that the great sciatic nerve had incurred injury in its course, from some similar mischance, to which the soft structures of childhood render it more especially obnoxious. These conjectures, as well as others that might be made, of course are more or less hypothetical, but nevertheless seem calculated to throw some light on the etiology of the complaint.

I shall put on one side those cases of infantile paraplegia which arise from incurable organic lesions, scrofulous or otherwise, and proceed to speak of infantile paraplegia arising from, or presumed to arise from, spinal concussion, or temporary injury of the sciatic nerve. From the cases to which I shall presently advert, it appears to me that these last sources of paralysis may spontaneously, or by remedial intervention, wholly cease. If the interval, however, prove long, the muscles, from disuse, become in so far incapable of performing their part as organs of station and locomotion, and the individual remains as if paralytic, when in reality he is only so in appearance. The cause of the primary paralysis, perchance, is removed, but the effect, muscular incapability, namely, remains.

The first case which I shall adduce is that of a little boy, four years old; he could walk, but with much difficulty, and very slowly, lifting each foot as if he were taking it out of a close-fitting boot, or out of adhesive clay. On pointing to the child; the mother bewailed its lot, as incapable of alleviation or cure. I suggested that she should give it plain nourishing food, attend to its bowels, practise assiduous frictions and shampooings over the spine and limbs, bathe it daily in cold water, and as soon as might be, in the sea. After passive or active exercise I desired that the little fellow should be laid on his couch, and not suffered unduly to fatigue himself. In a few weeks good effects were apparent; and in six or eight months, for I speak from memory, and cannot be exact as to a day, the child was completely exempt from all debility, and could run, jump, and play like others of his age. I now lost sight of him, and learned some time after, to my great regret, that he had perished of scarlatina.

The next child was the son of a shoemaker, rather younger than the preceding. The parents had considerable intelligence for their station, and conceived they had done wonders for their little boy, in procuring a pair of those absurd and villanous contrivances which bear the name of steel boots. After some explanation and persuasion, the father and mother consented to lay the "boots" past, and to commence upon a course of passive exercises, frictions, shampooing, and cold aspersions, as before. The child rapidly improved. I have not seen it very recently, but believe that it is alive, and so far as regards its limbs perfectly well.

Case 3.—It is now some months since was asked to visit a young lady, whom I found labouring under acute hysteric sternalgia. She suffered extremely, but I was

fortunate in relieving her by some of those mixtures with valerian, and so forth, which it is usual in such cases to administer. I had seen her on a previous occasion, and then had become aware that she was very lame, and unable to stand or walk without great difficulty. Her friends were grateful for the relief which this young person experienced, and I thought there would be no impropriety in inquiring into the cause of the lameness just adverted to. They readily informed me that the patient had begun to walk at the usual period (some eighteen or twenty years previously), but owing to some inscrutable cause had all at once ceased to be able to do so. I stated that it was possible that some relief might be afforded, and expressed my willingness, of which they gladly availed themselves, to examine into the case.

Upon inquiry, I found one limb, up to the hip, which was considerably developed, completely spent and attenuated; in fact, not thicker than that of a child of three years of age; the foot was small and completely flaccid, and there was no protuberance in the region of the calf. The integuments were everywhere flaccid and very cold, and a considerable protuberance subsisted at the knee, where a steel boot, between four and five pounds weight, reaching as far as the pelvis, had been attached. By means of this contrivance, doubtless assisted by the action of the glutei muscles and those of the trunk, the other leg serving as a fulcrum, was this poor girl slowly and painfully enabled to move about.

I carefully examined the spine, which was well formed, free from all unnatural curve or deviation, and could bear to be felt and percussed in every part, without the patient wincing. The leg, also, I was glad to find, was of the same length as the one on the other side. In other respects the body was well developed, the constitution free from serious disease.

This certainly was a case as little calculated to yield hope of a favourable result as there could well be. It was possible, nay, probable, that the primary source of the local paralysis had long ceased to act, but the muscles, the flexors, extensors, abductors, and adductors, alike were reduced to the condition of mere shreds. How was it practicable to develop them, or regain contractile powers which had been disused from infancy? Still, there could be no harm in trying, and try I accordingly did.

The patient was altogether too fat, and consequently heavy on her limbs; I therefore restricted her allowance, both in quantity and quality; made her exercise twice a day on a horizontal bar erected for the purpose, to incipient fatigue, and have the whole surface, the spine particularly, sponged daily with cold water, and well rubbed afterwards. Passive exercise was assiduously resorted to during all waking intervals; the spent and withered limb was flexed and extended in every joint—the the hip, the knee, the ankle chafed, rubbed, and gently pinched; it was at the same time rotated, adducted, and abducted, her sisters taking it in turns to do so. The steel boot worn ,and altered from time to time, for so many years, was at the same time relegated to the garret, never to be resumed again.

After these doings she was usually pretty tired when night came on, and slept soundly, to resume her tasks, into which she entered with the utmost zeal, the following morning. The power of using the limb at first was almost nothing, but by degrees it became greater; she came to flex and extend, to rotate, and use lateral motion; all this, of course, very feebly, but each day brought some slight improvement, perceptible to our inquiring vision. Her efforts at the horizontal bar sufficed to keep the leg warm, which it had previously required the fire and thick coverings of flannel to do.

I need not detail the gradual progress of my persevering patient. Suffice it to say, the limb gained progressively in circumference; superficial veins came to display themselves on the surface; a small but not unsightly calf grew upon the leg; the foot became larger and less flaccid, and she could with a little assistance support herself erect, and even proceed a little in advance. Subsequently the lump disappeared, and she could walk a dozen paces without the knees coming in contact. At present, after four or five months treatment, she walks every fine morning in the open air alone, and has ventured on foot across the square where she lives; has surprised me by opening the hall-door for me, and can go up and down stairs, formerly a weary task, with ease, holding on by the balustrade. At first I confined her to the drawing-room floor, a provision now no longer requisite.

I thought it right to blister the lower portion of the spine several times; each blister usually produced a peculiar thrill down the limb. Subsequently, I sprinkled a small portion of strychnia, mixed with white sugar, on the raw surface. This at first produced no sort of effect in the convalescent limb; but so soon as the muscular tissue was developed, it was curious to observe that the twitchings and contractions which strychnia usually induces ensued. I have ordered my patient to the sea-shore, and to bathe daily in the open sea, while she continues her varied exercises as before. The hysteric attacks, which once or twice suspended our operations, have wholly ceased, and the locomotive powers are progressing day by day, without the least tendency to remain stationary, much less relapse.

I do not think it unreasonable to expect that this remarkable case will end in a complete recovery; indeed, I feel quite confident on the subject, as does my interesting

patient. Thus a case of partial paralysis, of nearly twenty years' standing, which, short of similar measures, would indubitably have persisted for life, has been virtually removed, and the patient raised to the enjoyment of the pleasures, immediate or prospective, belonging to her sex and station. The issue falls in no wise within the chapter of lucky accidents, since the treatment was based on grounds which the result goes far to prove were correct. The case, however, with those which precede, will, I trust, help to establish a new position in practical medicine, as well as serve to rescue from neglect a class of diseases too generally esteemed incurable.

Belfast, May 14, 1843.

ON THE

TREATMENT OF CYNANCHE TRACHEALIS.

To the Editor of THE LANCET.

SIR,—I send you the following account of my treatment of cynanche trachealis, and the success that has attended it, or, rather, that has been derived from it, in hope that it will induce medical practitioners in Europe, particularly in Great Britain and Ireland, to employ it likewise. I have been at home since last spring, in consequence of bad health, from over-practice, two of our professors being ill. I am, Sir, your most obedient servant,

W. ROBERTSON, M.D.,
President of the Medical Faculty and Professor of the Theory and Practice of Medicine in the University of M'Gill College, Montreal, Canada.

George Cottage, Edinburgh,
April 28, 1843.

Soon after entering into practice I was surprised at the number of deaths which occurred in cases of croup, considering the nature of the disease being merely inflammation of the mucous membrane lining the glottis and upper part of the trachea, and seldom attended with any morbid change in the structure of the parts; except the secretion from the affected surface, or part, being so altered as to form a membrane on the inflamed surface with occasionally a slight degree of ulceration.

I always doubted any benefit being derived from the constant, or almost constant, application of leeches; on the contrary, the difficulty often experienced in stopping the hæmorrhage from their wounds, the consequent debility produced, and the alarm caused to mothers and other attendants was, as might naturally be expected, followed by bad effects, other remedies being but little attended to. Living in Canada, where, until very lately, leeches were expensive, and often not to be procured, when depletion was likely to be of service, I bled from some of the veins, and thought it afforded more relief than the leeches. When the child's arm is so fat that the veins at the bend cannot readily be got at, the applying a ligature round the arm, close to the wrist, and dipping the hand in warm water, seldom fails of raising or making a vein visible, from which a sufficient quantity of blood can be abstracted; or one of the feet may be had recourse to. When the patient arrives at three or four years of age, and the pulse is full and hard, the skin hot and dry, with other symptoms of fever, the effect produced on the general system by the sudden abstraction of blood from a vein is far more beneficial than in the lacerations required by the leeches before the required quantity is taken away. On being appointed professor of obstetrics and the diseases of women and children, about twenty-three years ago, my attention was naturally called emphatically to such diseases, as well as the line of practice demanded.

On maturely reflecting on this complaint, while preparing notes for my lectures (and preparing a first course is no joke), it struck me that instead of the bleeding by leeches bringing the fluid from the diseased part, or being local bleeding, it rather produced a contrary effect; for the arteries supplying the outer part of the skin on the neck and chest do not come from the same trunks as those supplying the mucous lining of the larynx, glottis, trachea, &c., and instead of attracting or drawing, from an increased flow to the surface where the leeches are applied, the contrary effect is produced; for it is well known when a leech is applied, and begins to draw blood, a quantity of cold watery fluid oozes from its surface, and repels the blood to the internal parts, and to vessels different from those they are obtaining it from. The long time required for blisters to produce a decided effect upon the skin, and the agony in many instances they cause, from their irritating effects on the urinary organs are well known, and the symptoms of the disease are increased by the crying of the little sufferer.

I made up my mind to try in a few cases a different plan of treatment, having been successful in changing the treatment in many cases of ague (of which I shall say something on another occasion). Having prepared a few ounces of a powerful and actively stimulating liniment; on the first call, and that was not long in coming, the weather being damp and cold, I took some of the solution of the tartarised of antimony besides the liniment. The child being stout, an antimonial emetic was instantly administered, which produced a most beneficial effect. When young, or weak, or of a delicate constitution, a large proportion of ipecacuanha is given with the tartrate,—that, of course, diminished, according to the child's age

—it operates quickly. Warm water ordered to be in readiness for a bath if required. The moment the emetic has ceased acting, or nearly so, the liniment is applied to the neck and upper part of the chest, by moistening a piece of fine flannel, lint, or cotton wadding with the stimulant,—these liniments, for I use more than one form, vary as to the appearance of the skin, age of the patient, &c. A very good composition is about equal parts of oil of turpentine, compound camphor liniment, and olive oil: if the child is young, less turpentine. Occasionally iodine or ammoniacal preparations are employed; or tincture of cantharides; mustard sometimes, but its effects on the skin are often severe. If it does not redden the skin and cause acute pain in from two to five minutes, it must be strengthened. When once it acts freely, a very short time will answer. The inflammation should extend from the top of the thyroid cartilage to the top of the sternum, two or three inches broad above, and wider below. As soon as small vesicles occur and a very red skin, with the papillæ much enlarged, the rubefacient should be removed, and replaced by a light, soft poultice of bread and milk, or water, and some oil or lard, which must be changed or cleaned occasionally. The patient to be soothed and pleased by trying to play with it, instead of crying or being dull; the less use they make of the nasal organs the better; a little liquorice to keep in the mouth, and, if the cough is troublesome, pectoral mixture, with a very small quantity of opiate preparation, either compound tincture of camphor, or one of the salts of morphia. Care must be taken in administering these, not a very large dose; or any, if there is not much irritation; the stimulation of the liniment is often of service. Soon after the stomach has recovered from the effects of the emetic, calomel is given, and repeated every two, four, or six hours, and in doses proportionate to the age and strength of the child, narrowly watching the gums, lest much increased action be produced. When the least redness or swelling of the gums or throat is observed, stop for some hours, and watch its constitutional effect. The bowels to be freely opened with castor-oil, rhubarb, tartrate of potassa, or some such medicine; when once freely cleared out a free aperient state is quite sufficient. If the skin is dry and hot, a warm bath is a good application; but if the patient's surface is moist, and itself weak or languid, we must be cautious in its application, taking care to cover the body the moment he is taken out, and instantly dry it with towels. If the medicines administered internally, or externally applied, do not produce the desired, or, at all events, expected effects, they must be changed, and others of somewhat a similar kind administered. Should any other disease, local or constitutional, be present, or come on or appear during the attack, it must be properly attended to; proper temperature, &c., being adopted all the time. Under this treatment very few patients have been lost. One, indeed, died, whom I saw within twelve or sixteen hours of the attack, and who was the last of a family that had lost eight children with the disease,—on one of whom tracheotomy was performed. This one was mentioned as being the only death under my care from the first moment, and being in attendance on the mother at the time, and for some days before, all that was likely to be of service was employed. Scrofula was hereditary in the family.

During convalescence the greatest care and attention must be paid to the patient; no exposure to cold, or damp, or any sudden transition of temperature; the neck, chest, and feet to be kept warm; and the diet simple, or of a nature found to agree and be proper for infants or children of the same age; air and exercise in the arms, walking, or in a carriage, according to circumstances.

Before concluding this interesting subject, I would not only recommend, but strongly urge, on all practitioners, the importance of instructing parents, friends, and nurses as to the nature and very peculiar sound of the voice, but particularly on crying, laughing, or coughing; that any intelligent person, after once hearing and being informed of the nature of the disease of which the peculiar sound was indicative as threatened, and the serious consequences likely to ensue if the premonitory signs be neglected, might be able to give instant notice of the approach of the disease.

MEDICINAL USE OF SAFFRON.—In several cases of obstinate chlorosis that had not yielded to preparations of iron, in one case of puerperal fever in which digitalis and bleeding had failed, and in two cases of chronic artero-phlebitis, Dr. Morgante, of Verona, reports that he has employed saffron with the greatest success, commencing with doses, in the form of pills, amounting to sixteen grains in the twenty-four hours, increasing the doses until the quantity is doubled. As to the manner in which this medicine acts—it is reported to be particularly effective in cases of increased action of the capillary vessels, and analogous in its effect to the more active preparations of iron. —*Memoriale della Medicina Contemporanea.*

FACIAL NEURALGIA.—An ointment composed of veratria, one part to eighty parts of lard, has been found very useful as an external application in cases of facial neuralgia. But the preparation is much more efficacious if made with rancid instead of fresh lard, which is probably owing to a salification and greater solubility effected in the veratria by the agency of the free acid in the fat.—*Revue Scientifique.*

PRISON DIETARY.

THE FOOD ALLOWED IN THE HERTFORD GAOLS.

To the Editor of THE LANCET.

SIR,—In the "Regulations for Prisons," recently issued by Sir James Graham, a table of "Dietaries" is given, which is intended to serve as a guide for the formation of the dietary of each prison. In the "Regulations" table the prisoners are divided into *ten* classes. Such a division may answer for *very large* prisons, but in those of an average size it would render the cookery, and other things connected with it, too complex and expensive to be easily carried into effect.

Taking the dietary recommended by the Secretary of State as a basis, I drew out the following more simple table, for the consideration of the visiting magistrates of our county gaol. They immediately approved of it, and, by their recommendation of it to their brother magistrates at quarter sessions, it was readily adopted. It has since received the sanction of the Secretary of State, and is now in use at our gaol.

As the *principle* of the dietary recommended by Government is expected to be applied immediately to all prisons in England and Wales, and as the Hertford county gaol may be considered to contain about an average number of prisoners, it is probable that the following classification would suit the majority of such establishments. Its objects are to simplify the classification, and to secure the health of the prisoners, by allowing them a *sufficiency* of food, composed of proper *proportions* of animal and vegetable materials.

I may state that our dietary is inserted at length in the last "Report of the Inspectors of Prisons," lately presented to both Houses of Parliament. Speaking of it the inspectors remark,—"The dietary is quite sufficient; in some respects it is higher than that recommended by the Secretary of State for prisons generally, but it is not at all too much, considering the low position of the prison, and the strict discipline and close confinement which is enforced."

I believe that only a few prisons have yet decided upon the details of their dietaries, according to the principle contained in Sir James Graham's order; I therefore send you ours for publication, if you consider the subject of sufficient interest, in the hope that it may form a guide, in some degree, for the formation of others.

It is to be presumed that surgeons of gaols generally possess the confidence of the visiting magistrates; it cannot, therefore, be doubted that the latter would pay every attention to the recommendation of the medical officer in a matter so important and so intimately connected with the medical department of the establishment as that of the diet of the prisoners. The power of the surgeon is very great under the laws and regulations of prisons, and the public have a right to expect that he will use it in such a manner as will best secure the health of the prisoners, without, at the same time, losing sight of the interests of those who have to support these prisoners.

The medical officer, by proper and timely representation, has it in his power to cause the removal, or, at any rate, the mitigation of *very many* of the evils connected with prison discipline, now so loudly complained of. It is absurd to suppose that the magistrates have any wish to starve the prisoners, thereby destroying their health permanently, and rendering them burthensome to the parishes ever afterwards. Such a thing would be quite contrary to their own interests; and I can bear testimony that it is not the fact, as far as regards this county, at any rate, and it may be supposed that men of the same class, and of similar education, are pretty much the same in all counties. I am, Sir, your obedient servant,

JOHN DAVIES, M.D.
Surgeon to the Hertford County Gaol, &c.

Hertford, May 20, 1843.

DIETARY OF THE HERTFORD COUNTY GAOL AND HOUSE OF CORRECTION.

Breakfast daily, for every class:—A pint and a half of skilly and eight ounces of bread.

Supper daily, for every class:—One pint of gruel and eight ounces of bread.

Dinner on Sunday for every class:—One pint of gruel and eight ounces of bread.

Dinner on Week-Days.

CLASS I.—Not exceeding one month, *without* hard labour:—One pint of gruel and eight ounces of bread.

CLASS II.—Not exceeding one month, *with* hard labour:—Monday, Wednesday, and Friday, one pint of gruel and eight ounces of bread; Tuesday, Thursday, and Saturday, one pint of soup and eight ounces of bread.

CLASS III.—Exceeding one month, and not exceeding three months, *with* hard labour; also solitary, by the order of court; misdemeanants of the first division; prisoners for trial; destitute debtors; likewise exceeding one month *without* hard labour:—Monday, Wednesday, and Friday, four ounces of *cooked* meat, without bone, eight ounces of bread, and eight ounces of potatoes; Tuesday, Thursday, and Saturday, one pint of soup and eight ounces of bread.

CLASS IV.—Exceeding three months, *with* hard labour:—Monday, Wednesday, and Friday, five ounces of cooked meat, without

bone, eight ounces of bread, and one pound of potatoes; Tuesday, Thursday, and Saturday, one pint of soup and eight ounces of bread.

N.B.—Females to have six ounces of bread where males have eight ounces.

Skilly.—Three-quarters of a pint of new milk, three-quarters of a pint of water, one ounce of oatmeal, and salt sufficient.

Soup.—One pint of water, three ounces of meat without bone, one ounce of flour, one ounce of onion, and salt and pepper sufficient.

Gruel.—One pint of water, one ounce and a half of oatmeal, one ounce of onion, and salt sufficient.

MEDULLARY SARCOMA CURED BY CAUSTIC POTASS.

M. LALLEMAND'S TREATMENT OF ERECTILE TUMOUR.

THE "Journal de Medec. de Lyon" reports the case of a child, ten months old, which was the subject of a medullary tumour on the left cheek, about an inch in diameter, indolent, and apparently consisting chiefly of capillary vessels. Compression to reduce it had been tried in vain, and the introduction of needles, by the removal of which internal cicatrices were to be set up, according to the plan of M. Lallemand, was prevented by the opposition of the friends of the infant. A practitioner now employed a solution of caustic potass, which was kept constantly applied by the aid of adhesive plaster; and after various eschars had successively fallen off in the space of a year, no trace was perceptible of the tumour that had existed. The practitioner in question (Dr. Fouilhoux), with praiseworthy moderation, hesitates to believe that similar treatment, though successful in this instance, would be so in similar fungoid growths in any situation, but considers that it is likely to be adapted only to such as have a cutaneous origin.—*Gaz. de Hôp.*, April 11.

The treatment of medullary tumours above alluded to, as proposed and adopted by M. Lallemand, is well developed in the following case:—A young lady, eight or nine years of age, had an erectile tumour of the face. The disease commenced by a small red swelling near the centre of the lower lip, and gradually involved the whole of the lip and a part of the chin; and on any emotion being experienced by the patient the tumour enlarged with surprising rapidity. M. Lallemand penetrated this unsightly formation in every direction with long and fine needles, fastening these in their situations by means of waxed threads. "Next day (says he) the whole of the morbid tissue was of a bluish or violet-red colour; inflammation had been set up in every portion of it," and it tended speedily towards abolition and cure. M. Lallemand continues, "I have lately seen this young lady who is approaching puberty, and of remarkable beauty. The lower lip is restored to its healthy condition, and preserves its powers of motion intact; not a trace exists of the growth which formerly augmented with such rapidity, nor in a lapse of three years has a return of the disease been threatened; nor does any trace of the operation remain, a circumstance not wonderful, since this consisted only in the introduction of a hundred fine needles throughout the tumour, and was unattended with any external solution of continuity. Compare the result of such an operation in a fine girl with that which attends the complete ablation of the lower lip!"

M. Lallemand admits that in many cases he has preferred the employment of potassa pura and other caustics to the above treatment, or even the actual cautery; and he signalises a case in which he used the latter with success to an erectile tumour at the base of the tongue. But it matters not what means are employed so that the object is effected of setting up an acute inflammation in the tumour that may finally convert the erectile into fibrous tissue, or arrest the nourishment it receives, by the closure of its arterial vessels.—*Archives Gen. de la Med.*, April, 1843; see also the same work for 1835, 2nd series, vol. viii., p. 5.

NEW CLASSIFICATION OF MEDICINES.—M. Mialhe, in the "Bulletin de Therapeutique," states that his researches have led him to conclude that the greater number of substances introduced into the economy act chemically either mediately or immediately on the serum of the blood, some *coagulating* and others *fluidifying* its albumen. In the class of coagulants are ranged all tonic, astringent, and styptic agents, as most of the mineral acids, a great many of the metallic salts, tannin, creosote, ergot of rye, &c. The class of fluidifiants comprises all true diuretics, with many alteratives and general excitants, including most of the vegetable acids, ammonia and its salts, the iodides, sulphurets, and alkaline chlorides, &c. But some medicines, which at first act as coagulants, afterwards become fluidifiant; this is the case with bichloride of mercury. Others, which have not any perceptible action on the albumen when first introduced into the circulation, become afterwards coagulant in a high degree. Of this class is ergot of rye. This substance, according to Mialhe, having after a time effected a thickening of the albumen, ultimately produces a firm coagulation "or rather a process of organisation more than simple coagulation." By this action Mialhe explains all the known effects of the ergot; and he supposes the agaric used for stopping bleeding the champignon, &c., to exert a similar coagulant agency.

A Practical and Theoretical Treatise on the Diagnosis, Pathology, and Treatment of Diseases of the Skin, arranged according to a Natural System of Classification. By ERASMUS WILSON, Consulting-Surgeon to the St. Pancras Infirmary; Lecturer on Anatomy and Physiology in the Middlesex Hospital School of Medicine, &c. &c. London: J. Churchill, 1843. 8vo. pp. 446.

DURING the last half-century the study of cutaneous diseases has been cultivated with much diligence and considerable success, by various able inquirers, both in this country and on the continent; yet never, until the appearance of the work before us, has that study been placed on the only scientific basis on which every branch of pathology must eventually rest, namely, the anatomy and physiology of the textures implicated in the morbid actions. This has probably arisen from the fact that we have only very recently attained to such a knowledge of the *intimate structure* of the dermoid textures as might promise any permanency to a pathological system founded on that structure and the functions connected with it.

Mr. Wilson observes, with truth, that the classification of Willan, meritorious as it is, and extensively adopted as it has been by succeeding writers, bears much the same relation to one based on anatomy and physiology as the artificial system of botany bears to the natural. The student will decidedly find, in the classifications of Linnæus and Willan, the most obvious means of identifying a cutaneous disease in its mature stage, or a plant in the state of efflorescence, but neither by the one nor the other classification will he be enabled to ascertain the natural and true relations of his object to others of the same kind; and, when he meets with the nascent or declining disease, or with the plant of which the parts of fructification are not yet developed, or have withered and disappeared, the artificial system, in either instance, fails him, even as a means of identifying the object set before him. In deviating from the classification of Willan, Mr. Wilson has, however, wisely adhered, as far as possible, to the excellent nomenclature of that distinguished writer.

The dermis, its glands, and follicles, are the seat of all those changes which constitute cutaneous disease. Hence our author establishes four primary divisions, namely,

I. Diseases of the dermis.
II. Diseases of the sudoriparous glands.
III. Diseases of the sebaceous glands.
IV. Diseases of the hair and hair-follicles.

I. The complex organisation and diversified functions of the DERMIS render it liable to a greater variety of diseases than its dependent glands and follicles. These diseases Mr. Wilson arranges under the following heads:—

1. Inflammation of the dermis.
2. Hypertrophy of the papillæ of the dermis.
3. Disorders of the vascular tissue of the dermis.
4. Disorders of the sensibility of the dermis.
5. Disorders of the chromatogenous functions of the dermis.

II. The SUDORIPAROUS GLANDS have but recently been made known by the researches of Breschet and Roussel de Vauzeme, and were first delineated in this country in the anatomical plates edited conjointly by Dr. Jones Quain and Mr. Wilson. Their pathology has not yet been separately investigated, but sufficient is known, relative to their secretion, to justify a division of their diseases into such as give rise to,

1. Augmentation of secretion.
2. Diminution of secretion.
3. Alteration of secretion.

III. The SEBACEOUS GLANDS are subject to numerous diseases which our author distributes into five groups, namely, those arising from,

1. Augmentation of secretion.
2. Diminution of secretion.
3. Alteration of secretion.
4. Retention of secretion.
5. Inflammation of the glands and adjacent tissues.

IV. The HAIR AND HAIR-FOLLICLES present a variety of abnormal and morbid states, which admit of arrangement under the six following heads:—

1. Augmented formation.
2. Diminished formation.
3. Alteration of colour.
4. Diseases of the hair-pulps.
5. Diseases of the hair-follicles.
6. Abnormal direction of the hair.

The principles which have guided the author in the subdivision of these classes and orders, and the individual maladies included under the ultimate subdivisions of his classification, will be rendered intelligible by the following table which we transcribe:—

I.—DISEASES OF THE DERMIS.

- **Inflammation**
  - Congestive
    - Specific: Rubeola. Scarlatina. Variola. Varicella. Vaccinia.
    - Non-specific: Erysipelas. Urticaria. Roseola. Erythema.
  - Effusive
    - Asthenic: Pemphigus. Rupia.
    - Sthenic: Herpes. Eczema. Sudamina.
  - Suppurative: Impetigo. Ecthyma.
  - Depositive: Strophulus. Lichen. Prurigo.
  - Squamous: Lepra. Psoriasis. Pityriasis.
  - From Parasitic Animalcules: Scabies.
- **Hypertrophy of the Papillæ**: Ichthyosis. Tylosis. Clavus. Verrucæ. Cornua.
- **Disorders of the Vascular Tissue**: Vascular Nævi. Purpura.
- **Disordered Sensibility**: Hyperesthesia. Pruritus.
- **Disordered Chromatogenous Function**
  - Augmentation of pigment: Nigrities. Pigmentary Nævi.
  - Diminution of pigment: Albinismus. Vitiligo.
  - Alteration of pigment: Ephelis. Lentigo. Chloasma. Melasma.
  - Chemical Coloration: Oxide of Silver Stain.

II.—DISEASES OF THE SUDORIPAROUS GLANDS.

Augmentation of Secretion	Sudatoria.
Diminution of Secretion.	
Alteration of Secretion	Abnormal Odour, Colour, &c.

III.—DISEASES OF THE SEBACEOUS GLANDS.

Augmentation of Secretion		Stearrhœa.
Diminution of Secretion.		
Alteration of Secretion		Ichthyosis Sabacea.
Retention of Secretion	Duct Open	Comedones. Sebaceous Accumulations. Small Sebaceous Tumours (*Molluscum Contagiosum.*)
	Duct Closed	Sebaceous Miliary Tubercles. Calcareous Miliary Tubercles. Serous Cysts. Encysted Tumours.
Inflammation of Glands and adjacent Tissues		Acne. Sycosis.

IV.—DISEASES OF THE HAIRS AND HAIR-FOLLICLES.

Augmented Formation	Pilous Nævi.
Diminished Formation	Alopecia. Calvities.
Alteration of Colour	Canities.
Disease of the Hair-pulps	Plica Polonica.
Disease of the Follicles	Inflammatio Folliculorum. Favus.
Abnormal Direction	Trichiasis. Felting.

To say that this classification is perfect would be to declare more than is true with reference to any nosological arrangement that has yet seen the light, but we are only doing justice to Mr. Wilson in pronouncing it unquestionably sound in its leading principles,—as little objectionable in its details as could be expected from the state of knowledge on the subject,—and, withal, by far the best classification of cutaneous diseases that has yet appeared.

Describing, farther, the contents of the work, we may state that chapter the first presents the most complete and lucid view of the anatomy and physiology of the dermoid textures which we have ever read, and forms an extremely useful introduction to the study of cutaneous pathology. Chapter the second is devoted to rubeola, scarlatina, variola, and vaccinia, of all of which excellent descriptions are given. Mr. Wilson's view of the general relations of these diseases is very peculiar. He not only regards small-pox, varicella, and cow-pox, as modifications of the same disease,—a tenet in which many agree with him, and which we ourselves rather incline to adopt,—but he revives and embraces the old doctrine of the virtual identity of measles, scarlatina, and small-pox. He institutes the following comparison between scarlatina and rubeola:—

"Between scarlatina and rubeola, the closest analogy undoubtedly subsists, and when the natural characters of the two affections are considered, the analogy approaches almost to identity; thus, both are inflammations of the tegumentary surface of the body, internal and external; both are accompanied by a cutaneous efflorescence, involving the vascular rete of the dermis; both are liable to be succeeded by serious affections of the viscera into the structure of which mucous membrane enters as a constituent part; both appear during the prevalence of the same epidemic, engendered apparently by the same infection; one may follow on the other as a consecutive disorder; both are infectious, and both are contagious. In practice alone is it necessary to distinguish between these exanthemata. We will, therefore, inquire what are the distinctions which we are enabled to establish between them?

"Scarlatina.	*"Rubeola.*
"1. Precursory symptoms of one day duration.	"1. Precursory symptoms of three days' duration.
"2. Mucous membrane of the eyes, nose, and fauces, red and inflamed, without secretion; pain and soreness of throat; no cough; no expectoration.	"2. Mucous membrane of the eyes, nose, and fauces, red and inflamed, with increased secretion, coryza, sneezing, &c.; dry cough at first, subsequently expectoration.
"3. Eruption on the second day of the fever; invades the entire surface of the body in three days; disappears by the end of the seventh day.	"3. Eruption on the fourth day of the fever; occupies three days in invading the entire surface of the body; disappears by the end of the eighth day.
"4. The efflorescence occurs in large irregular patches, or is more or less generally diffused; is of a bright scarlet, compared by Willan to a 'boiled lobster's shell,' and frequently interspersed with numerous small red papulæ.	"4. The efflorescence occurs in small, crescentic, and circular patches, with intervening unaffected portions of the skin; the colour is darker than in scarlatina, with 'nearly the hue of a raspberry,' and interspersed with numerous small red papulæ, disposed in clusters.
"5. Odour resembling old cheese.	"5. Odour, sweetish, until the decline of the eruption, then sourish.
"6. Principal sequelæ; anasarca; inflammation of the joints; gangrene; chronic bronchitis; ulcerations of fauces; conjunctivitis; otitis; abscess of salivary glands; chronic diarrhœa.	"6. Principal sequelæ. The same as scarlatina, with the exception of anasarca, inflammation of the joints and gangrene.
"7. Exfoliation of the epidermis in laminæ.	"7. Exfoliation of the epidermis in furfuraceous scales.
"8. Less infectious and contagious than measles.	"8. More infectious and contagious than scarlatina.
"9. Rarely attacks the same person more than once.	"9. Frequently attacks the same person twice.

"The differences above stated amount at most to one of *degree*, the infection being the same in both disorders. Thus, while both are constituted by inflammation attacking the same textures of the body, scarlatina, during its *first stages*, is more rapidly and actively determined to the cutaneous surface, the mucous membrane, in an equal ratio, escaping the violence of the inflammatory action. The contrary is the case with regard to rubeola; here the cutaneous determination is tardy and partial, while the mucous affection is gradual, severe, and prolonged. During the *second stages*, on the subsidence of the cutaneous congestion, the mucous membrane may suffer more or less in both, according to a variety of circumstances, such as the greater or less exhaustion of the morbific influence in the skin, the state of the nervous system, &c. These stages have no natural course in either disorder, new and accidental, or previously existing conditions, determining the resolution of the inflammation, or its attack upon some weak point of the mucous membrane."

There is, doubtless, some weight in these observations, though, for our own part, we still believe the poisons which occasion the two diseases to be perfectly distinct. We have certainly seen cases in which it was impossible to say whether the patient laboured under measles or scarlatina; but in those cases both diseases appeared to be present, and our impression was, that it had been too hastily assumed that they might not, in some rare instances, coexist. With respect to the identity of the poisons of measles and scarlatina with that of small-pox, the author merely states his opinion in the preface. He has done well not to argue the point, as further observation and reflection will probably lead him to form a different conclusion.

In chapter the eighth, which is on "Inflammation of the Dermis induced by Parasitic Animalcules," Mr. Wilson enters minutely into the subject of scabies. He believes that the sole and invariable cause of that malady consists in the presence of that species of acarus concerning which there has been so much controversy as to give origin to a work called "Acaro-machia," lately published in France. The following we believe to be a very accurate account of the ravages committed by this little parasite:—

"The vesicles, which are few and scattered, bearing no proportion to the number of the acari, and little relation to their seat, present some differences in form and character, respective of their position. Thus, in the thin epidermis of the lateral surfaces of the fingers, they are distinctly conical and acuminated; on the wrists and other parts of the body they are frequently more or less rounded, and resemble the vesicles of ecze-

ma; while in the latter situations they are also variable in size. The vesicles differ in reference to their contents; in those of a conical form the contained fluid is transparent and viscous; in the rounded vesicle the fluid is also transparent, but in some it is more or less opake and puriform. The pustules are present only in severe cases, or in persons with an extremely sensitive skin; they are generally psydracious, and vary in size, from the small pustule of impetigo to the larger pustule of ecthyma.

"When one of the early vesicles of scabies is examined with attention, a minute spot or streak may be observed upon some one point of its surface. This is the aperture originally made by the insect on its first entrance beneath the epidermis, and from this spot or streak a whitish line may be traced either in a straight or a curved direction, into the neighbouring epidermis. The whitish line is the cuniculus, or burrow of the acarus; it necessarily varies in length, being sometimes as much as five or six lines in extent, and at its termination, under a slight elevation of the epidermis, the little inhabitant lies concealed. The acarus may be easily distinguished by the experienced eye as a small dark point at the end of the cuniculus, and if a thin capsule of epidermis be raised in this situation with the point of a needle, the little creature is brought into view. It should be needless to remark, that eyes must be properly selected for the manipulation, and a bright light carefully chosen.

"The spot or streak which is here described is not met with on all the vesicles, for the same animal may excite a series of these in its course; and a number may be developed in the vicinity of its habitation, while in the primitive vesicle alone — that formed by the entrance of the acarus—it is, that the trace of its entrance can be expected. The aperture, again, does not communicate with the interior of the vesicle; it is the too close neighbourhood of the little *grubber* that acts as the cause of formation of the vesicle; the vesicle is consequently a provision of nature to protect the dermis from the nearer approach of the *arator*, and the vesicle is formed with the judgment which usually marks nature's operations—namely, before a defensive provision would be too late. The acarus scabiei therefore, is *never* situated within the vesicle or within the pustule, and there is no communication between the vesicle and the cuniculus."

Chapter the fourteenth, which treats of diseases of the sebaceous glands, is exceedingly valuable, embracing not only an elaborate analysis of all that is known on the subject, but several original and interesting contributions from the author. Under the head of "altered secretion," Mr. Wilson describes a variety of disease which has hitherto been confounded with ichthyosis, and to which he gives the name of *ichthyosis sebacea*.

"In addition to simple increase in quantity, it occasionally happens that the secretion of the sebaceous glands is also altered in its quality; when this is the case, the secretion spreads upon the surface of the epidermis, and forms a thin layer, which dries and hardens, and breaks in the direction of the linear markings of the skin, into small polygonal portions, corresponding in form with the areæ of the compartments bounded by these cutaneous lines. The small polygonal divisions are increased in thickness by the accumulation of fresh sebaceous secretion, they become discoloured from exposure to dust and dirt, and they assume a brownish or greyish tint, approaching more or less to dirt colour. In the latter state, the small masses have the appearance of scales (ichthyosis sebacea), closely adherent to the epidermis, hard and tense in texture, and presenting various degrees of thickness. This affection may occur upon any part of the body, but is most frequent on the face, particularly on the forehead and the nose, upon the abdomen, and upon the flexures of joints; indeed, upon all those regions in which the greatest number of sebaceous glands exist, and which are most protected from the friction of dress. The scales are sometimes cast from time to time, particularly during the summer season, and give place to others formed by successive concretion; at other times they remain adherent for months, and even for years.

"This affection of the sebaceous glands is generally unaccompanied by signs of local inflammation. There is, in many cases, no redness, and no heat, and when the scales are thrown off, the skin is natural both in colour and texture; in others, the skin is congested and thickened; it is studded with numerous apertures of sebaceous ducts, and frequently painful. By accumulation, the scales obstruct the mouths of the excretory ducts, and the latter become much distended. The disease is rarely accompanied by constitutional symptoms, but in a few cases when general, some degree of gastro-intestinal irritation may be present.

"In an instance of this affection which fell under my observation about ten years since, I had the opportunity of examining the skin after death of the patient from visceral disease. In this case, the scales were remarkable for their thickness; after being well washed, they were greyish in colour upon the surface, but white beneath, and evidently consisted of concreted sebaceous substance. On removing a portion of the epidermis by maceration, the ducts of the sebaceous glands and hair-follicles were found distended with inspissated white secretion, and had a very beautiful and bril-

liant appearance, projecting like cones of pearl from the under surface of the membrane. The dermis presented a number of small deep pits, corresponding with these dilated ducts. The mouths of the distended excretory ducts opened upon the surface of the epidermis, some immediately beneath, and in the middle of the scales, and others by their borders. In the former situation they could be seen as small white points through the scale, and still more evidently when the epidermis was separated by maceration.

"From the careful examination of this case, of which a preparation is now before me, and of other cases which I have subsequently observed, I have been led to the conclusion that the scales, in this disorder, increase in thickness in two ways, first, by additions to the free surface, by means of the secretion poured out in the linear furrows of the skin, and, consequently, between the scales; and, secondly, by additions successively made to the attached surface by the effusion of inspissated secretion beneath them. In the preparation before me the growth of the scales by both of these processes is distinctly evident."

The seventeenth and concluding chapters contain two very able dissertations, one on the natural history of the itch animalcule, the other on that of the *acarus folliculorum*. The latter is now, for the first time, fully and accurately described, and magnified delineations of it form a frontispiece to Mr. Wilson's book.

In concluding our notice of this work, we feel justified in saying that every part of it is replete with information distinguished by completeness and accuracy. The author shows a thorough mastery of his subject, both literary and practical, and has shown by his writings an indisputable claim to take a high rank in the profession as an authority on diseases of the skin.

GLANDERS
IN THE
HUMAN SUBJECT TEMPORARILY CURED BY IODINE.

The following case occurred under the care of M. Ludiche, a practitioner in the Prussian territories:—A coachman, fifty years of age, and of a strong constitution, felt himself attacked in March, 1837, by pain and feebleness in the right arm, hoarseness, dry cough, and anorexia. He remained, however, for two hours afterwards exposed to cold wind and a hail-storm. The face, neck, and hands, parts the most exposed to the weather, soon became the seat of acute burning pain; next day pain was felt in the neck with difficulty of deglutition; and eight days afterwards an eruption appeared, accompanied with much nervous prostration, fever, thirst, cough, dysphagia, dyspnœa, and lumbar pains. The parotid region and the backs of both hands were covered with deep red and hard papulæ, similar in appearance to the eruption of small-pox at its commencement. At the end of a few days the pimples had become as large as peas, and filled with fluid, which, having become absorbed, the epidermis fell off in whitish scales. Small scattered tumours had previously appeared on the hands, but these did not suppurate. The case was at first treated as a rheumatic affection, with purgatives, emollients, &c., but the weakness of the patient precluded bleeding. Under this treatment the fever, redness of the throat, &c., subsided, leaving, of all the former symptoms, only the eruption and difficulty of deglutition; but the patient continued to enfeeble and emaciate. Mercurials were tried, but with no other effect than salivation. At length in the sixteenth week from the beginning of the disease, it was ascertained that one of the horses formerly driven by the patient had been glandered six months previously, and that he had groomed the animal himself, by which he had placed himself within the pale of infection. M. Ludiche now employed iodide of potassium and tincture of iodine in the treatment; applying the latter remedy directly to the mucous membrane of the throat and mouth. After a few days the external eruption had disappeared, and the pain in the neck, &c., had become less. The use of the iodine was continued with alkaline baths; and at the end of three weeks a decided improvement had taken place, deglutition was easy, scarcely any eruption existed, and the appetite had returned. The cure was completed by August; and for four months afterwards it remained perfect. But the man having then been exposed for a night to cold and wet weather, all the symptoms he had formerly experienced returned; the eruption had the same seat as before, and was now accompanied with itching. After the febrile symptoms had been put an end to, iodide of potassium was again employed, and after the patient had taken a few doses the eruption subsided, and the papulæ fell off as scales. On the cessation of the medicine, the eruption was renewed, but it again disappeared in twenty-four hours under the influence of the iodide. The patient was afterwards lost sight of by M. Ludiche, so that the actual termination of the case is not recorded.—*Journal des Connaiss. Med. Chir.*, March, 1843.

THE LANCET.

London, Saturday, May 27, 1843.

No wonder that those medical excrescences, the Dubs and Pures, never loved us. Of all vicious practices that have come in our way none has received more fatal knocks on the head from our resistless club than the "*consultation* practice," and it is now nearly gone to the dogs. And well it deserved the fate. In cases of particular danger, great difficulty, or deep responsibility, the facility of obtaining counsel and assistance from men of great experience, skill, and sagacity, is, doubtless, inestimably advantageous, both to the practitioner in ordinary attendance and the patient, and no wise man will undervalue it. We admit the utility of consulting other practitioners; the error lay in calling in the wrong parties—not, really, men of great experience, skill, and sagacity, but a species of puffers and quacks, insolent fellows who *made believe* to be the "heads" of the profession in learning, tact, and practice. The imposition was very gross, and everybody wonders now how the profession so long allowed itself to be humbugged into an opinion that none but regular, pure, "consulting surgeons" and "physicians" could relume the fading lamp of life, or appropriately walk in at the last moment to see it go quite out.

How came all this about? Why was it that an arrangement on behalf of the sick, so useful and desirable in theory, failed to accomplish its legitimate ends in practice? Take a case which used to be of hourly occurrence. A youthful gentleman, newly graduated at one of the English universities, and freshly arrived in town, after having passed what is called an "examination" before certain pompous noodles in Pall-mall East, plants his name and additions on a door—*verba minacia fixo ære*—and straight sets up for a consulting physician. With what wonderful stock of *experience* does he commence practice! His peculiar position entirely shuts him out from that general range of professional observation which alone supplies a sound experience. What practical acquaintance with disease does he obtain at any period in his whole career? He becomes a consulting practitioner, alike devoid of experience, and incapacitated for attaining it. At very best, even in his *education*, he has been initiated into only one half of medical science, beyond the verge of which he gropes wholly in the dark.

Or suppose him to be a "pure surgeon." Crippled in one arm of medicine, and seeing, as it were, with one eye only, truly such a man is admirably fitted to enlighten his brethren who are in general practice, studying and pursuing the whole science, administering the whole art of medicine! We have had recently some forcible illustrations in public of the qualifications, ability, and medical sagacity, of several members of this class, bulletin-writers to the lamented Mr. Drummond and the Duke of Sussex, having at last the unfortunate duty to fulfil of recording the necroscopic examinations of their patients. It certainly does happen here and there that we have a consulting practitioner who has, at some period of his life, engaged extensively in all the principal departments of medical practice, and attained such an amount of professional knowledge and skill as renders him worthy of being consulted by his professional brethren; but the greater part of the whole tribe of "Pures," whether medical or chirurgical, have required rather to consult than be consulted. So far from being competent to guide the other members of the profession through the difficulties of practice, they would themselves have been fit pupils in the school of general practice.

Such then have been the causes of inefficiency in our consulting practitioners. A young man of average talents, perhaps well read, in every department of his profession, has just finished his education. Eschewing general practice, he starts as a candidate for consultation fees, either in the depart-

ment of physic or surgery—no matter which—adopting the usual orthodox plans of gradation in his new status, which, however, have none of them anything to do with his mental qualification for the position of consulting surgeon or physician. But there he is, in May-fair or Broad-street, or elsewhere, according to his fancy, ready for the guineas, and especially polite to all the general practitioners who have the honour to make his acquaintance. His presumed superiority in medicine is wholly self-assumed; his real value is precisely that of any decently dressed young man who has commenced practice among the drawers and bottles of a regular shop; yet infinitely less than that of a intelligent and diligent surgeon who has been ten years in general practice. To "call in" such an one to a case of doubt or jeopardy is not simply ridiculous; it is dangerous and unwarrantable. However lecture-crammed and book-taught—to summon to the bedside, over the heads of experienced and well-informed surgeons in general practice, such a student—not a *practitioner*—is the height of folly.

Admitting the utility of consultations in medical practice, "How," say the advocates of grades and divisions in the profession, "How do you propose to constitute the order?" We answer, that there should be no such "order" at all. The supposed necessity for the *order* is the very source of blunder in consultations. Let consulting practitioners become such by the simple fact that the profession consults them for their skill and experience. If individual practitioners possess an amount of knowledge and talent which entitles their opinions to more than usual consideration, certain it is that their professional brethren will find them out, and solicit their assistance in cases of difficulty or peril. Let the profession make as many consulting practitioners as it pleases on the ground of their individual merit and usefulness. At present the parties *measure themselves*, and in doing so use a pretty long strip of personal conceit.

It is scarcely necessary to do more than allude to this subject as one would to a point in history, for the custom now prevails in all sensible quarters of consulting the best surgeons in general practice. Common sense has got the victory over a very mischievous custom, and in this case is sure to keep it.

We endeavoured last week, in this division of our Journal (p. 271), to put the question of "Hydropathy" in its proper light. The facts were few, the arguments naturally brief, the subject soon exhausted. But it has a fellow fraud, and that imposture comes as a proper pendant to the other. We allude to "Homœopathy."

Even at this late hour of its career, the public overlook the meaning of that term, always regarding it as the title of the little-dose school.

According to the principle laid down by the author of the system, it implies the doctrine that deranged animal functions are remediable by the application of such agents as would, if administered in the normal state, produce similar derangements. The assertion of Hahnemann, who instances the practical application of this theory in the long-established stimulating treatment of burns and scalds, leaves no doubt as to this construction. In the common phraseology of old women, "Fire draws out fire," and the difference between homœopathy and medicine is this, that the former goes along with the disease, the latter wars against it. We shall not, in making a few remarks on this theory, enlarge upon the various coincidences which are cited, *à priori*, to justify its adoption, but shall prove that a definite combination of atoms is as necessary to produce specific action in *the animal*, as in the *chemical laboratory;* and that a specific disease is not the result of *indefinite combinations*, just as gases are not formed by the indefinite mixture of certain ingredients. Take the results of the combination of oxygen and nitrogen. Observe how widely their offsprings differ from each other in every quality. The formation of a specific

gas is not the mere result of the juxta-position of particular elements, but of an unknown power, which permits the union of bodies only under certain circumstances.

We have in this instance an opportunity of examining several compounds which differ from each other in colour, in smell, and in external characters, as they do more essentially in the effects that they produce on the animal economy. Who would, *à priori*, suppose that atmospheric air, nitrous oxide, and nitric oxide, are merely compounds of the simple elements, nitrogen and oxygen? The differences which exist between those combinations are too well known "to warrant us in doing more than stating the "fact, that chemical combinations are the re-"sult" of elective affinities, without which election matters do not combine chemically.

Well, then, is not something of the same nature necessary for the production of specific disease,—for we believe in specific disease and in specific remedies. Organic chemistry confirms that doctrine. We choose for illustration that affection which seems to justify, at first sight, a belief in the reality of homœopathic pathology, viz., *scarlatina*, On this malady great stress has been laid by the Hahnemannists, to prove that certain agents, applied to the body, will produce morbid action in a state of health, and that the normal state will be restored by the application of the same agents.

If belladonna be taken inadvertently, nausea, vomiting, fever, and scarlet eruption, will ensue. Hence belladonna is regarded as being naturally the remedy for scarlatina. Belladonna is used, also, as a prophylactic against the contagious influence of scarlatina. We have known whole families subjected to a course of this drug, where a child in the nursery has been affected with scarlet fever. That is surely a most useful and comprehensive style of practice, a kind of medical trinity, which recognises in the same agent the means of creating, annihilating, and preventing a particular malady. But is a scarlet skin, accompanied by the aforementioned symptoms, sufficient to constitute the specific disease, *scarlatina?* We have seen enough of this disastrous complaint to prevent us from regarding it as merely an eruptive fever, attended with high efflorescence, and to induce us to recognise in its mode of attack and fatality, and in the miserable sequel, some specific morbid poison, not produceable by a dose of belladonna, the effects of which are no more comparable with the real disease than is the redness of the body that is produced by flagellation with the eruptive fever of the same. Let us trace the progress of the two affections in all their stages before we admit their identity.

Will belladonna produce a scarlet eruption in all individuals to whom it is administered, or does not the occurrency rather come under the category of *idiosyncrasy*, as we see it result from eating salmon, shell fish, mushrooms, &c.? How many deaths are upon record of the disease produced by belladonna? Or what proportion is there between the mortality of the adventitiou and the natural diseases? Do patients labouring under the former sink in the course of forty-eight hours from the onset,—often with but little suffering, slight fever, scanty eruption, no change in the pupil, th sensorial powers only slightly deranged, but with difficult deglutition, livid fauces, and swollen, purple fingers,—as they do but too frequently in the latter? Is the desquamation that follows the efflorescence a slight furfuraceous peeling off of the cuticle, or does the skin come away, like a leather glove, in any cases of the belladonic fever? Are ulceration of the tympanum, exfoliation of the bone, violent inflammation of, and morbid secretions from, the eyes and lachrymal passages, and general dropsy, ever produced by belladonna? Is the disease resulting from belladonna communicable by contagion?

If all these symptoms and sequelæ have never coincided in the progress of the two diseases we have no grounds upon which t establish their identity. If they be not the

same diseases, they are not produced by the same causes, nor is the remedial agent for a spurious complaint specifically applicable to one that is specific. Genuine scarlatina can never be produced by a combination of belladonna with the animal secretions, as a specific gas cannot be formed by the mechanical mixture of *uncombinable* elements.

Again, the similarity of the symptoms which are produced by mercury to those of the mercurial disease, prove, it is said, the propriety of administering mercury for the cure of syphilis. That mercury will cure this affection we have ample proofs, but no evidence exists in favour of its adoption from the fact of its causing eruptions which simulate those of the venereal disease; for as twenty grey horses will never make a white one, so a variety of similar symptoms does not constitute identity of diseases. If, then, the diseases be not the same, the argument falls to the ground, the principle fails upon which the whole scheme is based, and there is no medical signification in the term "homœopathy."

Take another view of the subject. Suppose that the remedy which we would apply for the cure of a malady should be capable of producing that malady if exhibited in a healthy condition of the system. What principle, either in domestic affairs or in pathology, warrants the conclusion that by purposely adding fuel to fire we can control a furnace? If a disease be characterised by a variety of distressing symptoms, is there any philosophy in adding to their number or intensity by agents that are productive of aggravation? Can we subject life to the laws of inorganic substances, and,—upon the principle that similarly electrified bodies repel each other,—generate a positive morbid electricity, in the hope that when the already positively electrified morbid pole is brought into contact with the similarly artificially electrified end, the two will fly off at a tangent? We might then recognise the "unity of disease, the chrono-" thermal principle, the electric state of the "brain; and class love and pregnancy among "the varieties of intermittent fevers." Just as rational would be the idea of a patient, attacked by several mortal maladies at the same time, escaping death in the struggle which the diseases exert among themselves to get the upper hand of each other.

The term "homœopathy" is, as we have already intimated, lost sight of in the practice of the system, just as the name of Butler is submerged in his hero Hudibras. In its general acceptation the public only recognise the infinitesimal dose system, which, with that same public, constitutes the great charm of "homœopathy." It has the other signification among those adepts who teach their unfortunate children to present their burnt fingers to the fire, and make them keep the digits there so long as the increase of the pain can be tolerated by the sufferers. The principle is directly opposed to the ancient belief that "Chaque mal est gueri par maladie contraire," the idea upon which the old King Charles acted when he invoked the virgin aid of Joan of Arc to remedy the disasters accruing to the state from his amours with Agnes Sorel.

We have seen in practice too many good effects produced by counter-irritation, to abandon it as a remedy for disease. Its use is by no means free from objection. Indeed, in many cases it is unadvisably employed, and it always requires great discrimination as a remedial agent. Too frequently, however, the abuse of a thing is urged in argument against it, instead of drawing deductions from its use. Very many affections we have seen reduced by the external application of tartar emetic, and find no counter argument against its employment in the fact that St. John Long produced mortification and death by rubbing the spine with muriate of antimony. We deny not, either, the efficacy of minute doses of medicine. A world of mischief has been produced before now by long protracted doses of powerful medicines in chronic cases, many of which would, on the contrary, have been benefited by non-interference with medicine, or—which comes to to the same thing—by homœopathic treat-

ment. "It can, at least, do no harm," is the rock of salvation in this system. This apology it is which adds converts to HAHNEMANN; but although the infinitesimal doses be negative for harm, can they be positive for good? Is the drowning man,—incapable, from physical exhaustion, of saving himself,—more indebted to the one bystander who will not lend him a helping hand than to the other who pushes him down, and thus curtails his agony? But, in reality, this practice does effect positive good, inasmuch as that by avoiding causes of aggravation, by following a rational regimen, great assistance is afforded to the *vis medicatrix naturæ*. By one of the many perversities of our nature, we implicitly conform with particular regulations when they are associated with something that is *new*, although we should turn to them a deaf ear when prescribed for our use under ordinary circumstances. In all those cases, therefore, where patients with nervous and chronic complaints have little to hope for from medicine, and much from the removal of exciting causes,—from the stimulus of novelty, and from faith in nonentities,—the homœopathic system is invaluable.

It requires time and observation to become *au fait* at homœopathy. As regards the infinitesimal doses of medicine there seems to be a near approach to the position that it acts upon the system in an inverse ratio with the quantity administered. We may apply to it what Queen DOLOLOLLA said to TOM THUMB, when complaining to her,

"My grief is great, because it is so small,"

To which the obdurate queen replied,

"Then twould be greater were it none at all."

There is no exaggeration in this. The criticism is justified by the observation of a learned German professor, who recommended a few drops of water in which pure gold had been immersed, in a case of syphilis. To our objection that gold was insoluble in water, he replied, "With us, all matter is soluble in water."

The doses of medicine, however, present the least difficulty in studying this system of atom-entity. A variety of circumstances are required to be taken into consideration, at a great sacrifice of time, and much mock-profound application is required from its disciples. Not only may the infinitesimal doses produce, according to the homœopathic practice of medicine, the most monstrous effects, but it is necessary to ascertain the precise state of the system at the time when they are administered. Changes are continually taking place in the susceptibilities to impression, all of which are influenced by the physical position of the body. There are a thousand *niceties* to be considered, as regards even posture, and, still more, functional condition,—*Posture*, whether the body be dextral or sinistral, ventral or dorsal,—*Function*, "pediturus aut peditus."*

To close. We deny the truth of the principle upon which homœopathy is said to be founded, because—if from no other reasons,—it has failed in so many cases where it guaranteed success,—because it professes to be a *finality* measure,—and because the supposed cures are not the results of its operations, medicinally acting, but of the hygietic adjuncts, which, professing to co-operate with the drugs administered, are, in reality, the only and a sufficient means of cure.

CRITICISMS
ON
SIR JAMES CLARK'S REMARKS ON MEDICAL REFORM.

To the Editor of THE LANCET.

SIR,—You will much oblige me by inserting the following criticisms in your valuable Journal:—

Sir James commences by recommending the union of the Colleges of Physicians and Surgeons, in order to embrace in one great corporate institution *all* the members of the profession; and endeavours to show that this union might be effected *without* disturbing *the distinctions and grades* which are now recognised in the profession, and could not fail to promote harmony among medical practitioners generally. How this harmony is to be produced, and the distinctions and

* See Manuel de l'Homœopathie, Paris, 2 vols., 12mo.

grades which are now recognised in the profession, preserved, by the union of the Colleges of Physicians and Surgeons, I am quite at a loss to imagine. If this union were to take place, the general practitioner would receive his diploma from the same institution as the physician, and would be consequently on the same footing. How, then, would he feel towards a physician who might be called to consult with him, or, as it were, over him, in a case of which he had the management? Would it not give rise to ill feeling on both sides,—the one considering himself slighted, the other feeling that he possessed an equal amount of knowledge to that of the person who was called in to criticise his treatment? Would the public have as much confidence in the acquirements of the physician as heretofore?

Sir James says, "I expressed my belief that the practice of pharmacy ought to be separated from that of medicine." There are two great objections to this:—

First. The practitioner, when he required a medicine for immediate use, would be compelled to send to a chemist's for it, which, in many cases, would be exceedingly inconvenient. Sir James, however, admits that in villages and country districts the practitioner would require to keep such medicines as were necessary for immediate use, more especially in the treatment of acute diseases.

Secondly. The general practitioners are at the present time only remunerated for their services by the medicines which they send to their patients; and every medical man who has been long in practice, especially in the country, must be aware that persons generally do not like to give a fee to a *common doctor*, as they term them, without receiving some equivalent in the shape of medicines, &c.; if this custom was abolished there would be no distinction, in point of fact, between a physician and an apothecary.

On the point of preliminary education, I certainly think that the interference of the Legislature is decidedly unnecessary,—as the evil will remedy itself, and is at this moment doing so, which is generally admitted by all the medical boards. Is it likely, in this enlightened age, that a parent would intrust his family to the care of a medical man who, to use Mr. Guthrie's expression, could not spell very common words in his native language; or would a board of guardians to the poor employ such a person to alleviate their sufferings? I say, considering how much a medical man is criticised, *certainly not*. In the next place, if a very large amount of preliminary knowledge is required, it may very justly be supposed that a youth cannot commence his professional studies so soon in life as he otherwise would do, to say nothing of the expense of obtaining this knowledge. Then comes the professional education, occupying almost as much time as, if not more expensive than, the former. It, therefore, would require a very long time and a great outlay of capital to procure this education, the half of which has nothing to do with the profession of medicine. Taking these things into consideration, may we not infer that persons having time and possessing means to give themselves this education, will say, "I will not waste my time and capital in such a precarious speculation, and with such poor chances of being sufficiently remunerated hereafter." Again, those who do educate themselves in this way, will not (because more independent) pay so much attention to patients as has been given by persons depending upon the profession for a livelihood, and in the end the public will suffer generally, more especially the poor.

Sir James, again, remarks, "Had the scientific education of medical men been better attended to, the nation might have been spared the loss of *much human life* (listen to this, medical men; of what are you not accused by a *court* physician) and the fruitless expenditure of much treasure, and the public health might have attained a much higher standard than it has yet reached." Disinterested and amiable man! Did he entertain the same opinions when about commencing his professional career? If he did, how overwhelming must have been the thought of the responsibility of his situation, and how often must he have said within himself, "Oh that I had the wings of a dove, that I might flee from this slaughtering class, and be at rest." I am, Sir, &c.

P. O. L.

London, May 19, 1843.

*** We should have appended to this letter a few remarks, for it is doubtful whether the criticism or the production criticised is the less appropriate to the crisis. But the subject wanted discussion, on which account we have inserted the letter, intending to make some comments on both, when more space is at our command.

MEDICAL SOCIETY OF LONDON.

Monday, May 15, 1843.

Dr. CLUTTERBUCK in the chair.

THE DUKE OF SUSSEX.

THE PRESIDENT referred to the case of a late illustrious individual, as it was reported in the daily papers, to show, according to the post-mortem examination which had been also published, the little connection which existed between actual disease and the morbid anatomy exhibited. Bulletins had appeared day by day until the disease proved fatal, but these daily reports afforded no satisfactory idea of the nature of the disease; the patient was one day "better," another day "worse," or "stationary." When the

post-mortem appearances were published they threw no more light on the cause of death than did the daily bulletins. There was scarcely any disease in the cavities of the head and chest, and the only morbid appearance in the abdomen was a granular state of the liver. It was difficult to infer from the history of the case or the after-death appearances what the illustrious duke did really die from. He (Dr. C.) felt anxious to know more respecting this patient, because during life, although he had not the honour to attend him professionally, yet he had frequently come in contact with him in consequence of his being president of a dispensary of which he (Dr. Clutterbuck) was physician. Many years ago this illustrious individual suffered from severe attacks of asthma, in some of which his life was despaired of. He had seen him suffer much from this cause. These attacks left him many years before death, during which nothing remarkable was observed in his breathing. It was curious to find that nothing more was found in the respiratory organs than a little redness of the lining membrane of the air-passages. Anxious to know more of the actual symptoms of the attack of which he died, he had been in communication with one of the medical attendants of the duke, from whom he had learned that these symptoms were referrible to the brain, and that for several days before death the patient lay in a state of stupor. Yet dissection scarcely pointed to the head for an explanation of the fatal result. The small quantity of effusion, however, was, he (Dr. C.) thought, sufficient to show that increased arterial action had really existed, and yet the degree of effusion hardly carried with it the idea of danger. When he considered that the other parts spoken of exhibited nothing which would lead to a fatal result; that the lungs were healthy, the air-passages only being a little redder than natural, the heart not being diseased, but flaccid, and therefore less likely to be the seat of disease; the liver having merely a granular appearance throughout, he thought that we were driven back to the brain to explain the cause of death. Now we could not couple the apoplectic symptoms with the slight effusion between the membranes, as this was not sufficient to interrupt the important functions of the brain. Death, therefore, must have occurred from some cause which left no post-mortem appearances. He should, if called upon to explain the cause of death, refer it to a disordered vascular action in the brain, which interrupted and stopped the functions of that organ. This action it was known might exist and terminate fatally without the effusion of either blood or serum. This illustrious individual was predisposed to such an action, both by his figure and habits, the one being corpulent, and the others those of a *bon vivant*, though he did not either eat or drink to excess. He also led an indolent life. There had been also much distress of brain from long and repeated attacks of dyspnœa, which must have excited and disturbed the circulation in that organ, and which, by interrupting its functions, produced a state of stupor, and eventually proved fatal. The case, then, was one of apoplexy, from increased arterial action in the brain, without any actual rupture of vessel, or any actual apparent disorganisation.

Dr. T. Williams spoke of the unsatisfactory character of many post-mortem examinations, the appearances exhibited by which being often quite inadequate to explain the causes of death. He mentioned a case of apoplexy attended by paralysis of motion on one side, in which, after death, nothing was found to explain the hemiplegia. Small quantities of fluid were found in the ventricles and under the arachnoid, but it was equal on both sides, and was in such small quantity, that he had often seen much more in cases in which there was no paralysis. In the case of Richard Carlile, who was subject to attacks of asthma whenever he was in London, there was nothing whatever found after death to explain the circumstance.

Mr. Crisp remarked, that in fatal skin diseases very little was found in many cases to explain the cause of death. In some persons very little would cause death. In the patient's case mentioned by Dr. Clutterbuck, the coronary arteries and valves of the heart were ossified, and there was effusion between the membranes of the brain.

Mr. Linnecar animadverted on the loose manner in which the post-mortem appearances were described in the case under discussion.

After a few words from Dr. Willshire the society adjourned.

Monday, May 22.

Mr. Pilcher in the chair.

MR. BRUNEL'S CASE.

The President having hinted that Mr. Brunel's case afforded an interesting subject for discussion, Mr. Bishop remarked that the chink of the glottis, when spasmodically closed, could not be again forced open by the whole of the respiratory muscles, and that the sensibility of the larynx about the glottis, as was well known, was so great, that the chink was closed whenever it was irritated by any solid foreign body. With reference to Mr. Brunel's case, he said that as the half-sovereign had fallen upon the lower surface of the glottis by simply inverting the body, and that the chink had immediately closed without suffering the coin to escape through it, he was of opinion that it might have been extracted if an instrument, such as an elastic catheter, with

a sufficient aperture for the patient to breathe, had been passed by the mouth through the glottis, previous to the inversion of the body; and that by this method there would have been no danger of asphyxia arising, because the glottis could not close up the tube; and that if the piece of gold had fallen with its flat side towards the chink, the point of the instrument would have turned it edgeways, and suffered it to pass out by the side of the instrument. Mr. Bishop called the attention of the society to some cases which had been successfully treated by Desault, by passing an elastic catheter through the glottis in the manner he recommended. In the absence of this mode of treatment he thought that the distinguished surgeons who attended Mr. Brunel did perfectly right in opening the trachea.

Mr. CLARKE was unacquainted with any cases occurring in British surgery in which the employment of the forceps in removing foreign bodies from the trachea was successful, with the exception of the remarkable and interesting case recorded by Mr. Liston, in his "Operative Surgery," and another which occurred to Mr. Fergusson. In the first of these cases the patient, a female, nearly forty years of age, had got a piece of a mutton-bone entangled in the glottis whilst eating some hashed meat. In this position it remained for some time, when, during a violent fit of coughing, the bone became dislodged, but passed into the trachea, and eventually lodged in the bronchus. The operation of tracheotomy was performed, and the piece of bone successfully removed by means of a pair of forceps of a peculiar construction. No bad consequences followed, and the patient made a speedy and happy recovery. In the case detailed by Mr. Fergusson, in his excellent work on "Practical Surgery," the patient was a little girl, seven years of age, who had accidentally let a plumb-stone slip into the larynx. Tracheotomy was performed at the time, but the foreign body could not be seized, and the case was given up. Three months afterwards she came under the care of Mr. Fergusson. It was then ascertained that the foreign body moved between the upper part of the trachea and its bifurcation. The trachea was opened, and a slender pair of forceps introduced. The stone, during one of its movements, was dexterously caught between the blades of the instrument, and instantly removed. In a case operated upon by Mr. Key the patient died while the surgeon was attempting to remove a sixpence from the trachea.

Mr. DENDY thought the case of Mr. Brunel presented nothing so very extraordinary, and would have made no noise had it occurred to a less distinguished person, and under the care of less eminent surgeons. There did not appear to have been any steps taken in the case which any surgeon would not have taken, with the exception, perhaps, of the inverted position.

Mr. LINNECAR thought that the case, as detailed in the journals, presented at least one practical point, and that was the influence the opening of the trachea exerted in the removal of the coin. If that opening had not been made he believed that the coin could not have passed through the glottis as it did, because the patient would have been suffocated by its closing up that orifice in its attempt to pass through. The gravity of the coin in this case favoured its exit when the patient was in the inverted position. The cough itself would not have been sufficient to have brought the coin up to the glottis if the patient had been erect.

Mr. DENDY and Mr. ROBARTS could not think it possible that a bougie could have been passed through the glottis, as we knew that the slightest irritation of that part would produce a spasmodic action, and perhaps convulsions.

The PRESIDENT observed, that touching the glottis so as to excite irritation, and the rapid and dexterous passage of the bougie, were accompanied by quite different effects. In the latter proceeding, the glottis would not be called into spasmodic action if the operator were an adroit one. The case of Mr. Brunel was interesting from several circumstances; it was interesting in relation to the diagnosis formed respecting it, and from its being unique, as far as he knew, in the circumstance of a foreign body in the bronchus having passed upwards again through the larynx. It was not even common for foreign bodies to be removed from the trachea even by artificial means. He knew of only two cases (and these were those detailed by Mr. Clarke)* in which the operator had succeeded, by the introduction of forceps through an opening in the trachea, in removing foreign bodies contained in that passage. The diagnosis of foreign bodies in the trachea was by no means so easy as might be supposed. Even in Mr. Brunel's case some good surgeons had questioned whether the coin had, in reality, ever passed into the trachea at all. They believed, on the contrary, that it had got entangled in the vocal cords, and that the difficult breathing was thus accounted for, as was also the great relief that followed the making of the opening in the trachea. The coin being held in this situation it was expelled when the cough came on, during the time the patient was in the inverted position. It might be said, in opposition to this opinion, that the coin, if so situated, might have been seen or felt and he could not but think it would had it really been there. The only evidence that the coin was in the trachea was that the patient felt it move up and down in

* A case somewhat similar is detailed in the last vol. of THE LANCET, p. 808.—ED.

that passage. This symptom, at best a fallacious one, was the only evidence to guide the surgeon, for there were no stethoscopic or other signs to assist him in forming a diagnosis. He (Mr. P.) had never seen a case in which a foreign body had been taken from the trachea, or, when in that passage, that it had made its exit by the glottis. To show the difficulty of forming a diagnosis in cases of this description, Mr. Pilcher related the following case:—A little girl, two or three years old, whilst playing with a pebble, suddenly swallowed it, and it was thought it had passed into the trachea. The child, however, continued playing about in perfect health, and there was not the slightest indication afforded by the stethoscope that a foreign body was in the air-passages. She had occasional fits of coughing, and after one of these, more violent than usual, was brought to him. He wished to see her during a paroxysm; this, however, he did not do, for shortly after the child fell dead while in a fit of coughing. A rounded pebble was found in the rima glottidis, and there was a slight appearance of inflammatory action at the bifurcation of the trachea. It would appear that the foreign body had been playing up and down the trachea for weeks, and had at last become fixed in the rima glottidis. In this case a tracheal opening would have saved the patient. In the case of a child, three or four months old, who was being nursed under a hedge in a hay-field by a very young child, the infant was suddenly seized with a croupy kind of breathing, and its face became bloated. The child was treated for acute laryngitis, and died. A piece of cabbage-stump, lying obliquely between the vocal cords, was discovered after death. If the presence of this body could have been detected during life, laryngotomy could have been performed with a successful issue. These two cases had made an impression on his mind, and he felt determined, should a case warranting interference occur again, to resort to operative procedure. Some time afterwards he was requested by a surgeon to see a child who was supposed to have a tamarind-stone in the trachea. There were no symptoms of its presence, however, in this tube; the child remained well, and there was no irritation. An opening was made in the child's trachea and forceps introduced into the tube. No stone could be detected. The child fell into bad health, and acute bronchitis came on. It then became a question whether the bronchitis had not been induced by the opening in the trachea, and it was thought advisable to close it. Previous to this being done, however, the late Dr. Thos. Davies was consulted. He thought, after a most careful and minute examination, that the tamarind-stone had not passed into the trachea at all, and that the aperture should be closed. This was done, the child rallied for a short time, but soon sank and died. On examination after death the tamarind-stone was found imbedded in the upper part of the right lung, having passed through the right bronchus. The lung was one mass of disease, and in quite a pulpy condition. In this case the child might have been inverted, as was Mr. Brunel, but it was questionable whether the stone would have ever left the bronchus. So much for the diagnosis in these cases. The second circumstance of interest in Mr. Brunel's case was the subsidence of the irritation in the glottis caused by the contact of the coin immediately after the formation of the opening in the trachea. This would lead us to a good practical experiment, viz., the attempt to irritate the rima glottidis in an animal after opening its trachea. It would appear, however, that Mr. Brunel suffered more from the obstruction to the breathing caused by the coin when in contact with the glottis than from actual irritation. It might have been possible for the coin to have passed through the rima glottidis quickly, without producing any irritation. It was remarkable in Mr. Brunel's case that as the coin passed up and down the trachea it should not have found its way out of the opening in that passage; this was, indeed, the mode in which foreign bodies usually made their exit from the trachea in cases where tracheotomy was performed.

Dr. Risdon Bennett begged leave to correct the President in one or two of his remarks. The diagnosis was not founded entirely, as he supposed, on the sensation experienced by the patient. Immediately on the occurrence of the accident, a spasmodic cough, to which he was not accustomed, came on. After that, paroxysms of coughing took place at intervals. A pain was also experienced at the right side of the chest. With these symptoms he went to Sir B. Brodie, who, with the patient, was in doubt as to the presence of the coin in the trachea. Mr. Brunel went home, and leaning over the back of a chair he felt the coin move upwards to the glottis, and a violent coughing came on. He did not repeat the experiment, because quiet had been enjoined him. When placed on the inclined plane, which was the suggestion of himself, the same result followed. The experiment of the inclined plane was only once repeated, and that was at the time the coin was so happily ejected from the passage. The diagnosis was, therefore, formed on the fixed pain in the chest, the bronchial irritation, the otherwise healthy condition of the respiration, the swallowing being unaffected, and the sensation experienced by Mr. Brunel of the coin returning up the tube. It was not correct to state that the coin had been ever seen near the opening in the trachea, and the danger of the attempt to find it by the intro-

duction of instruments had been so great that it was not repeated after the first time. Even at the time of the last inversion of the body there was no appearance of the coin at this opening; there was a slight cough, and Mr. Brunel drew the coin from between his teeth. Until the occurrence of Dr. Roots' case and this, Dr. Bennett had thought the diagnosis of the presence of foreign bodies in the trachea more easy than he now did. In neither of these cases was there any difference in the respiration of the two sides of the chest. He could not help thinking, however, that some obstacle must have existed, though it might be very small indeed in the case of a smooth coin, but in the case of a portion of a bone there must have been some difference, if it only arose from the local bronchitis excited thereby. The diagnosis in the latter case had been the same as in that of Mr. Brunel. A symptom, however, had obtained in the case of Dr. Roots, and in that of a drill-sergeant living a door or two from Mr. Brunel, which was worthy of notice. This drill-sergeant had, many years since, the misfortune to let a rupee slide down the trachea to the right bronchus, where it now remained. He, as did, also Dr. Roots' patient, experienced a pain in the middle of the right side of the neck, corresponding with the lung affected. He (Dr. B.) did not attempt to explain this symptom. Dr. Stokes, in his work on Diseases of the Chest, had brought together a great number of cases in which foreign bodies had passed into the trachea. He (Dr. B.) had also notes of two cases in which Dr. Stokes had given an opinion in favour of the presence of foreign bodies in the trachea, in opposition to that of several other medical men who considered the cases to be apoplectic. The operation of tracheotomy was performed in these cases, and the lives of the patients saved. Mr. Brunel's life was, doubtless, saved by the opening in the trachea. The inclined plane Mr. Brunel had himself suggested, and much of the success of the case was to be ascribed to his mechanical ingenuity.

Dr. Chowne remarked that the medical men were pleased to see the coin ejected from the trachea, not only because it afforded relief to the patient, but because, notwithstanding their accuracy of diagnosis, they were fearful he would die, and no half-sovereign be found in the trachea. This information he had obtained from undoubted authority, from "one who knew everything." The case of Mr. Brunel was valuable as showing us that we had no certain sign of the presence of a foreign body in the trachea. We ought then to think the surgeon warranted in cutting into that tube whenever he had a strong apprehension that a foreign body was present in it. It was, at all events, a preparatory step in any future operation.

Dr. Bennett had been told by one of the surgeons in attendance that they had all the evidence of the presence of the foreign body in the trachea that they could have, with the exception of actually seeing it. With regard to the uncertainty of diagnosis in these cases, he (Dr. B.) thought this was the exception to the rule. The alteration in the respiration was generally so great that the case could not be mistaken. In Dr. Stokes's cases the respiration was so obstructed, and the cough so laryngeal, that only the presence of a foreign body could explain. He thought in all cases there would be such a diminution or obstruction to the respiration in that lung, over the bronchus of which the foreign body was situated, that no mistake would arise. The diminution would, of course, be modified by the kind of body situated over the bronchus.

Mr. Headland would willingly have submitted to an operation had such been thought necessary by either of the surgeons in attendance on this case, but he doubted whether an operation had been necessary in Mr. Brunel's case, or whether, in fact, it had in any way tended to its happy termination. It might be urged in favour of this opinion that the rupee had remained in the chest of the old soldier for many years without producing any inconvenience, and in the case of Dr. Roots the woman had ejected the bone through the rima glottidis without any operation having been done. He thought Mr. Brunel's case would have terminated as well without the performance of any operation. We wanted guides in our practice of surgery. In what way would Mr. Brunel's case tend to guide us? No one had explained how the operation had facilitated the removal of the coin, which did not come through the hole made in the trachea but passed through the glottis. Looking, then, to this fact, that the operation of tracheotomy was often a fatal one, and that in this case it did not appear to facilitate the ejection of the foreign body, he thought the case would have been quite as successful without its performance. It was said that the hole in the trachea had allowed the passage of air into the lungs, but there was no evidence to show this, and in Mr. Lewis's case such an opening was not found necessary. In Mr. Brunel's case much advantage was obtained by his courage as well as by his mechanical skill in its progress and termination. His steadiness and courage, no doubt, prevented much of the mischief which might have resulted from the operation. He (Mr. H.) would still maintain that our diagnosis as to the presence of a foreign body in the trachea should be unequivocal before we resorted to the use of the knife. The stethoscope surely ought to furnish us with evidence on this point.

Mr. Stedman related the case of a child, thirteen months old, who was suddenly seized with symptoms which the medical

man in attendance attributed to croup. The respiration was attended with a peculiar whistling noise. The symptoms suddenly subsided, and the child remained well for about ten days. Whilst out with the nursemaid it was suddenly seized with the same symptoms, and died almost instantly. On examination after death, a bead, about as large as a pea, was found in the rima glottidis. The whistling sound had been produced by the passage of the air through the hole in the bead.

ROYAL MEDICO-BOTANICAL SOCIETY.—*May* 11.

H. Gibbs, Esq., in the chair.

A communication from Dr. Houlton "On the Collecting and Preserving Roots for Medicinal Purposes," was read. The discordant opinions found amongst medical practitioners and authors respecting the medicinal properties of indigenous vegetable remedies, may be attributed in a great measure to want of information respecting the method of collecting, preserving, and manipulating these very important articles; as pharmaceutists have no sound practical guide to direct them, there is a great want of uniformity in their preparation, and consequently in their properties. To remedy this was Dr. Houlton's object, by laying down some safe rule for collecting and preserving roots. The directions contained in the "London Pharmacopœia" he condemns as uncertain and erroneous, and further shows that the instructions contained in the work of Dr. Bellingham and Dr. Mitchell are incorrect. He observes that *all roots should be taken up at the time that their leaves die*, as they then abound with the proper secretions of the plant. This rule has no exception; it applies to the roots of trees, shrubs, herbs, rootstocks, bulbs, corni, and tubers; and it includes that curious plant, colchicum, whose flowers only appear in the autumn, and its leaves and fruit the following spring and summer. Biennial roots must be taken up in the first year of their duration, as, when the leaves decay in the second year, their roots are either decayed, or merely dry woody fibre. Roots intended to be preserved should be dried as soon as possible after they have been dug up; the large tree roots, especially the more juicy ones, dry better in their entire state than when sliced.

UNIMPREGNATED OVI.

To the Editor.—Sir: In looking over The Lancet for March 18th, my attention has been arrested by the remark of "Gallus," relating to the unimpregnated fowl laying eggs. Permit me to make a few statements on the subject. Not only does the fowl lay eggs without the assistance of the cock, but the hen becomes much more prolific by being kept from the male. In this way I have obtained upwards of 250 eggs in a year from *one bird*. The deposition of ovi independently of impregnation I have myself observed also in the goose, the duck, the raven, the parrot, the pigeon, the canary, and the goldfinch, and I have no doubt that it takes place in all birds, as well as in many insects, particularly moths, butterflies, the house-fly, and the May-fly. If silk-worms, while in a state of chrysalis, be opened, they will be found full of eggs, which are not unfrequently deposited a few hours after they have assumed the moth state. Should this happen previously to their becoming connected with the male, they are, of course, useless. Knowing this, I used to keep the female chrysalises (they may be distinguished by their larger size and broader tails) in a cold place, to retard their transformation until some of the males had become moths. All fishes spawn, or deposit ovi, without assistance from the male, and the fecundation takes place after escape from the body. In frogs it is much the same, with this addition, that in the spawning season the males will be seen fixed on the backs of the females, tightly embracing them under the axillæ with their fore-legs, the pressure that they exercise being so great that the legs of the male will be found quite curved. This pressure is said to assist the female in depositing her ovi, and as they pass out of her body the male emits a fluid upon them that renders them prolific. The instinct which guides the frog in this act appears to be most blind, for sometimes four or five males will fix themselves around one female, and, by their united pressure, not only destroy her, but some of their own lives also. I have seen a frog fastened on the back of a toad. The snail and the black-snake will also deposit their eggs independently of previous impregnation. In short, in birds, fishes, reptiles, and insects, the male, instead of playing a material part in the formation of the ovum, has nothing to do with it, though the *semen masculinum* is required to make them prolific after being formed by the female.

As regards Mr. Girdwood's supposed analogy between the depositing of eggs in birds and the flow of the catamenia in women, I would remark that I thought it was pretty generally considered that its appearance was a proof that one or more of the ovi contained in the ovaries was sufficiently matured for impregnation, and that the establishment of that function merely prepared the uterus for the reception of the ovum. I believe it is well established that impregnation never takes place previously to the flow of the catamenia. How it is that Mr. Girdwood confounds the secretion, or the excretion, which you will, of the

uterus called the "catamenia," with that from which a new being springs, I am at a loss to know. The only cause that I can imagine for such a mistake is the common practice of using the erroneous expression, "impregnated uterus." It is the ovum, and not the uterus, that becomes impregnated. The uterus merely serves as a nidus for the development of the ovum. That such is really the case, "extra-uterine pregnancy" sufficiently manifests. I am, Sir, your obedient servant, GALLINA.

May, 16, 1843.

CASE RELATED BY MR. LEWIS.

To the Editor.—Sir: Having observed in THE LANCET of the 6th inst. the case of a "foreign body in the trachea for twenty months," reported by Mr. G. Lewis, of Woburn-place, and having been in daily attendance upon his brother during his stay in England, I was somewhat surprised at seeing the names of Messrs. Rowe and Keal mentioned in conjunction with the case, and having since learnt from Mr. Lewis the way in which their names became so introduced, I think it right, in justice to Mr. Lewis, to myself, and the profession, that you should be made acquainted with the facts. It is quite true, that Mr. Rowe saw the gentleman alluded to frequently, as he happened to live next door to my friend Mr. Lewis, and it is equally true that he showed great interest in the case, by calling nearly every day to inquire after the patient. It appears also that he very kindly undertook to take Mr. Lewis's letter to THE LANCET office for insertion; and then he thought proper to add his own name to the case, without the knowledge of Mr. Lewis, who assures me that he had not the slightest authority from him for doing so. Now, Sir, I cannot help thinking this somewhat unprofessional on the part of Mr. Rowe, and should wish to ask you if it is usual for medical men to take so much interest, and mix up their own names in the cases of their neighbours, merely because they happen to live next door. In justice to Mr. Keal allow me to add, that I believe that gentleman was perfectly ignorant of what Mr. Rowe had done until the publication of the case in THE LANCET. I am, Sir, your most obedient servant, HENRY SIDDEN.

Blackfriars-road, May 18, 1843.

*** Mr. Rowe seems to us simply to have added an additional authentication of the letter.

A GLUT OF DOCTORS.

IT appears that the profession is overstocked with practitioners in France as well as in England: more than forty French doctors in medicine having sometime since applied to the Government for free passage to the Marquesas Islands.

CASE OF TOTAL ABSENCE OF THE SPLEEN.

AT Vienna in 1838, the body of an infant was examined which had lived only three days, plunged during the whole of that period in incessant sleep. It had consequently never sought the breast; the meconium and urine had, however, been passed. Only a rudiment existed of the left forearm and hand, and the flesh of the other extremities had a want of tone and firmness. The abdomen and thorax were tumefied; but in most other respects the child presented little that was unusual in appearance, and the female genital organs were perfectly developed. But on opening the cavities the brain was found softened, and, together with its envelopes, gorged with blood, and a little fluid was present in the ventricles. The lungs were also gorged with blood, and a frothy secretion at their base; but the thoracic organs had a natural development. The abnormal appearances were mostly in the abdomen. Here the liver was found to extend almost as far into the left as into the right hypochondrium; and to its anterior and inferior border the transverse colon was attached in its entire length. The liver was attached also to the mesocolon, but less intimately. The small intestine held its ordinary position. There was no stomach, unless a small sac, thirteen lines in length by about one-third as wide, and situated at the angle where the œsophagus joined the duodenum, could be so called. There was a pancreas about sixteen lines in length, by four and a half in breadth; but *the spleen was entirely wanting*, and the large intestine had neither *sacculi* nor *appendices epiploicæ*. For further particulars the reader is referred to "Muller's Archiv.," &c., for 1842, p. 57.

We have before had occasion to advert to the fallacies liable to attend the deductions drawn from experiments performed on *live* animals; and the above case affords a suitable opportunity for again remarking that facts furnished by comparative anatomy, together with accurate observations on the defective performance of the animal functions in monsters, are the most legitimate sources whence to derive our conclusions upon points in physiology at present either dark or disputed.

HOSPITAL STATISTICS.—At the Hôpital la Charité, Paris, in the medical year 1841-42, 21 cases of diseased breast were received, of which 12 were cases of abscess, 8 of tumour, and 1 of eczema. Cases of hernia 17, of which 4 were crural and 13 inguinal; an operation was required in only 4 instances. Of diseases of the anus 28 cases; 2 of furuncular; 4 of deep-seated abscess; 4 of hæmorrhoids; 2 of syphilitic excrescences; and 15 of fistulæ.

NEWS OF THE WEEK.

THE poor-law commissioners have recently issued an official circular, answering the following question:—A surgeon (not an officer of a union) was suddenly called in to attend an accident in a district where there are a number of collieries and railways. The guardians desire to know whether they were authorised in such a case to pay the charges of the surgeon for attending the case. The commissioners replied that in case of sickness or accident requiring immediate attendance, and the services of the medical officer of the district cannot be promptly obtained, the relieving officer may employ any other medical man to attend the case. (Vide general order of the 21st April, 1842, Art. 20, No. 3.) They also think that in a case of urgency in which, in the absence of the medical officer, another medical man attends *without an order*, the guardians would be justified in paying him for such attendance, provided they subsequently decide that the person attended was at the time in a destitute condition. The amount of charge must depend upon the circumstances, and be settled between the medical man and the guardians. The fees prescribed by articles 10 and 13, of the general medical order, referred to medical officers only. The medical officer of the district should be directed to take the case under his charge as soon as practicable.

THE guardians of the poor in the Windsor union are, it seems, not particularly desirous to be distinguished for their humanity. A poor woman, whose husband is in gaol, and who has several children to maintain, having been brought to bed of twins, applied for relief to one of the overseers, who gave her two shillings—no great stretch in itself of parochial bounty. But will it be believed that on the overseer afterwards claiming reimbursement from the board of guardians his claim was *actually disallowed?*

DR. CLENDINNING stated, at a late meeting of the Medical and Chirurgical Society, that he understood a committee of the College of Physicians were at present actually engaged on a new edition of the London Pharmacopœia.

THE newspapers announce the death, on Tuesday last, of Mr. Tyrrell, surgeon to St. Thomas's Hospital. It occurred very suddenly, at the Auction Mart, in St. Bartholomew's-lane, just after the unfortunate gentleman had given instructions to a friend to bid for some property (East Lodge) at Acton, of which Mr. Tyrrell held the lease. Mr. T. was much esteemed by his friends, and his death has caused them very painful feelings. He was a man of average ability and acquirements, whose genius did not interfere with the plodding habits that help to make a fortune, and whose industry never carried him through a month's labour to enrich medical science from the stores of knowledge that laid before him in the immense hospital of St. Thomas. Indeed, his appointment to that institution was not obtained under any pledge to advance the general public interest in medical facts, and the governors will probably supply the vacancy which his death has made in the usual ignoble and unsatisfactory manner.

THE Twenty-ninth Report of the "Glasgow Royal Asylum for Lunatics," has this week been forwarded to us by Dr. Hutcheson, the physician to the institution. Among the one hundred and ninety-nine patients admitted in 1842, while fifty-four cases are stated to have been hereditary, congenital, or constitutional, only in one is the insanity ascribed to "organic disease of the brain;" and of twenty-seven various causes, to *intemperance* not less than forty-six cases are ascribed, nearly one-fourth of the whole admissions. As an alarm against drunkenness such a fact is very startling, but to trace insanity to its particular causes is more difficult than may be suspected. Nothing is more common at coroners' inquests than for inexperienced persons to hunt up reasons for the insanity which generally precedes suicide; yet—we speak from observation and experience—nothing is more fallacious than the conclusions at which the inquirers stop. The insanity, for instance, which is ascribed to "intemperance" should itself most frequently be set down as *the cause*, and not the effect. Dr. Hutcheson evidently has a sense of the truth of this allegation, for in the page adjoining the "causes of disease" he makes the following remark:—"The annexed table contains only what are called the *exciting causes.* I am convinced, however, that the *predisposing cause of at least two-thirds is a hereditary tendency to the disease.* This is a fact of great importance, and imperatively demands the attention of the medical profession and of the public. It is obvious that the most effectual method of checking the increasing number of the insane would be to modify or eradicate the hereditary predisposition to the disease." The report comprises several fatal cases, in which the post-mortem appearances are carefully described. We mention this for the advantage of those persons who seek for information on the subject. They are terminated with this remark:—"From the inspections detailed above, as well as from those given in last report, it appears that diseases of the thoracic and abdominal organs prevail to a great extent among the insane; thus verifying the observations of Esquirol and others on this subject. It is not, however, to be inferred that such affections are either the immediate causes or consequences of insanity. Indi-

rectly, they may be either the one or the other; but, without doubt, disease of the brain, either functional or organic, local or general, is in all cases the immediate antecedent of morbid manifestations of mind."—P. 60. The report is published at Glasgow by Rodderwick and Son.

THE "new world" journal called the *Australian*, has brought to our office in the "old world" the record of an operation for a deformity which the reporter (residing at Singleton, St. Patrick's Plains) grandiloquently announces as having "perhaps no parallel in the history of medical science," and which he has communicated to the newspaper, in consequence of "the absence, at Australia, of a medical journal for communications of this nature. It occurred in a child which the deformity rendered literally a horror to human nature, and a source of great affliction to its parents. The child," he continues, "is not quite three months old. In addition to a double hare-lip and double fissure of the palate, the centre portion of the upper jaw-bone, containing the sockets of two of the front teeth, instead of being placed and attached in its natural situation, was protruded through the fissure in the upper lip, firmly attached to, and projecting beyond, the nose; and above this, and connected with the tip of the nose, was situated the centre piece of lip, forming an hideous appearance. Dr. Stolworthy, of this town, removed this protuberance, as it was impossible to convert it into any useful purpose in repairing the deficiencies. The centre piece of lip, being too far removed from its fellows to approximate them, was removed with the jaw-bone, thereby reducing the case to one of single hare-lip, the operation for which was performed with the pins in the usual way. The operation occupied half an hour, owing to the loss of blood, which protracted the operation, by the delay required to recruit the vital powers. The boy is out of danger. On opening the portion removed perfectly-formed teeth were found. Great interest was excited in the town by the case." In the name of the prophet, *Figs!*

THE deaths from *typhus* in the metropolis registered in the week ending Saturday, May 13th, 1843, were, under fifteen years of age, 11; from fifteen to sixty years and upwards, 10; total 58. Of these 58 deaths, 3 occurred in Westminster; 5 in St. Marylebone; 3 in St. Giles; 6 in West London; 3 in Southwark; 3 at Newington; 3 in Lambeth; 4 in Greenwich (2 in the Dreadnought); and 15 in St. Pancras, of which 8 occurred in the Fever Hospital. Of 7 patients who died in the Fever Hospital, 3 came from St. Giles.

In the same week, of the 12 deaths registered from *small-pox*, 9 of the patients had not been vaccinated.

ON the 5th inst., George Tyler, a carrier, committed suicide in the yard of his employer, at Worcester, and was removed to the infirmary, where on the 12th he died. He had fired a pistol with the left hand, the ball entering the skull rather low down on the left side, passing through the posterior portion of the brain, which it tore up in its progress, and lodging against the skull on the opposite side. He was insensible in the infirmary, excepting when roused, and then talked rationally. While dressing the wound, Mr. Cole, the house-surgeon, found a small portion of lead near the orifice of the wound, between the skull and the scalp, which must have been shivered from the ball on its coming in contact with the skull. Upon making a *post-mortem* examination, Mr. Cole discovered the bullet battered in passing to where it was lodged. It was globular when discharged. Mr. Cole says that he never before met with a case where the brain had been so much injured and the person survived so long.

THE last Report from the House of Commons on petitions relating to medical affairs presented in the present session of Parliament, contains the following summary of those documents:—

Petitions against transferring the medical charities of Ireland to the control of the poor-law commissioners, 5, containing 79 signatures.

Petitions against the Irish Medical Charities Bill of last session, 4; signatures 79.

One petition for the appointment of a committee of inquiry into that bill, containing 2 signatures.

Petitions in favour of the bill, 6; signatures 344.—In favour of it, with modifications, 3; signatures 6.

Petitions for a Parliamentary inquiry into the state of the medical profession in Great Britain and Ireland, 8; signatures 195.

Petitions for medical reform, 8; signatures 14.

Some of these latter, and probably many of the former petitions, contained the signatures of one person each, made on behalf of bodies or associations of medical men. The whole number of petitions presented in 1843, relating to medical subjects, is only 35, containing 762 signatures. The last presented was "for inquiry," from the medical practitioners of Chichester, by Lord Arthur Lennox, containing 23 signatures.

IN the House of Commons on Wednesday last (24th of May), Mr. Henley having noticed the "epidemic diseases which had so often occurred in the Millbank Penitentiary," said he thought that these "must certainly have been super-induced by a short diet, because no officer of the prison who had *access to the kitchen* ever became affected with ailments." To this the Home-Secretary

replied that "the complaints did not arise from the low diet, but from the site of the prison. In most cases, long confinement and low diet proved fatal to the constitution of the prisoners. But the diet had been amended, and yet it was found that long confinement in that place would not answer. As a place of lengthened confinement, therefore, *the Penitentiary must cease*. But to what use could so large a prison be converted? It was well adapted for carrying into full effect almost every object of prison discipline, but, unfortunately, the site was unhealthy. The Government had thought (in its views respecting the classifying of prisoners) that the Penitentiary might safely be made a general depot for prisoners whom it was necessary to confine for terms varying from nine to twelve months, because it was not thought that during that short period the locality could have *any* injurious effect upon the constitution of the prisoners." It was "not *thought*," says Sir James. A few more experiments, therefore, are to be tried on human life in the prison. Condemn vivisection in brute animals after this. While on the subject of prisons and prison diet, we take the opportunity of saying that we shall, in an early number, insert a report of an inquest that was recently held (the first, and as yet the only one) in the Pentonville, or new Model Prison. The subject of separate confinement and diet in that establishment has excited very great interest and curiosity, both in and out of the profession, into both which subjects very particular inquiry was made on that occasion.

THE APOTHECARIES' LAW IN FRANCE.

THE Royal Court of Rouen has been lately occupied with the final decision of the following case:—The medical jury of that city had found in the house of an apothecary some deteriorated drugs, on which account the apothecary had been condemned in a fine of one hundred francs (4*l.*) and twenty-four hours imprisonment. Appeal was made to the Royal Court, the advocate of the apothecary pleading that the law did not punish the simple possession but only the sale of such drugs, referring to a decision to that effect of the Royal Court of Paris in 1834. The advocate-general, however, pleaded the 39th article of the law, 21 *germinal* an. II., and cited a decision of the Court of Cessation, and the ground of appeal in this case was overthrown.—*Gazette des Hôpitaux*.

LEGITIMATE GRATITUDE.

A STATUE of Berthollet is about to be erected at Annecy, in Savoy, the native town of that celebrated chemist. Let us hope that examples of this kind may one day be imitated in England, and men learn here to raise public monuments to the benefactors as well as the destroyers of the human race.

A PUZZLE IN CHEMISTRY.

THE following prescription was sent to be made up to an apothecary in Berlin:—

℞ *Flowers of arnica montana*, fifteen grains;
Boiling water, three ounces. Infuse, and add to the filtered fluid;
Powder of gum arabic, three drachms;
Carbonate of magnesia, eight grains;
Syrup of marsh-mallows (*guimauve*), six drachms. Mix.

It so happened that this mixture, after having been made, remained with the apothecary for several hours before being sent for, during which time it successively exhibited the following changes. The liquid, which at first had been of a yellowish tint, became gradually greenish, and, though originally clear, it terminated in becoming a cloudy mixture of a hue between olive and grass-green. The infusion had certainly been prepared with arnica flowers of good quality and unmixed with any insect eggs. What chemical changes accompanied the change of colour? This would seem to have been due to the action of the carbonate of magnesia; calcined magnesia effected a similar appearance, but only after a much greater lapse of time. The presence of the gum or the syrup of marsh-mallows was found by subsequent experiment not to be in any way the cause. Carbonate of potass added to infusion of arnica only deepens its yellow colour, and the same result is produced by the addition of liquid ammonia. Lime-water effects a cloudy appearance with a light yellow precipitate. Sulphur of copper also causes a cloudiness, with the production of a yellow-green tint.—*Med Zeitung*.

ANALYSIS OF ANGELICA ROOT.

AN analysis has lately been made by M. Buchner of the constituents of angelica root, which is found to comprise, besides an essential oil, a species of wax, a crystallisable resinous matter called *angelicine*, a peculiar acid termed *angelic acid*, with a bitter principle, resembling tannin, a gummy extract, and various salts. The *angelic acid* exists, combined with potash, in the alkaline liquor obtained by heating the balsam of angelica with caustic potash. The addition of sulphuric acid to this mixture causes the disengagement of a very strong and acid odour, and the deposition of the angelic acid in soft, oleaginous, but subsequently crystallisable masses, the product of which, when distilled, saturated in potash, and then redistilled with strong phosphoric acid, is the angelic acid in its purest state. This oily acid congeals at a temperature considerably higher than water, forming large, striated prisms; its smell resembles that of valerian mixed with acetic acid, and the taste is fiery as well as harsh. With the alkalies

and alkaline earths it forms soluble compounds, which, on the addition of the salts of silver and lead, give a white precipitate, and when mixed with a solution of chloride of iron yield a flesh-coloured precipitate, like benzoate of iron, and wholly different from the brick-coloured precipitate of angelicate of iron.—*L'Experience.*

THE DEVIL *versus* THE DOCTOR.

An unlucky doctor of Ghent had, a few weeks ago, a narrow escape with his life from the violence of certain Belgian rustics, who took great dudgeon because, forsooth, he placed more reliance in the exhibition of medicinal agents than of saintly relics! Here, then, was a battle between physic and divinity, the case (that of a poor epileptic boy) being one which the latter specially loves to take under her charge, as the devil is believed to be the great tormenter of epileptic subjects. How far the sage gossips of Olsena succeeded in beating the devil out of the patient we cannot inform our readers; but at any rate they beat the doctor out of the village.

INJECTION OF MILK INTO THE VEINS.

It was formerly supposed that this operation was inevitably attended by a fatal result. A commission of the French Academy of Sciences has, however, ascertained that in the frog and the dog (which live on highly nitrogenised food) the effects produced are supported without inconvenience. In the horse, indeed, the experiment often proves mortal.

Benefits of Homœopathy.—A contemporary states that *another* member of Lord Denbigh's family has fallen a victim to the Hahnemannic system of medicine, the third son of that nobleman having died after hooping-cough from an accumulation of mucus in the throat, which might probably have been removed by energetic treatment.

Society for Relief of Widows and Orphans of Medical Men.—The annual dinner of the above society, which was postponed in consequence of the death of the Duke of Sussex, the Patron, is now appointed to take place, as appears by the advertisement in The Lancet of this day, on Saturday, the 3rd of June. H. R. H. the Duke of Cambridge has signified to the stewards his intention to preside on the occasion.

ROYAL COLLEGE OF SURGEONS IN LONDON.

List of gentlemen admitted members on Wednesday, May 17, 1843:—T. Lloyd, T. Willis, J. J. Fox, S. S. Alford, J. H. Browne, R. Leack, W. Smith, P. Berry, E. Labrow, G. Y. Heath, B. R. Mudd.—Admitted on Friday, May 19:—C. Girdlestone, F. R. Manson, W. J. Price, W. McCheane, W. M. Pinder, J. Carter, H. Dixon, W. R. James, J. S. H. Williams, W. Davy.

BOOKS RECEIVED.

Medico-legal Reflections on the Trial of Daniel M'Naughten for the Murder of Mr. Drummond, with Remarks on Insanity and the Irresponsibility of the Insane. By Jas. George Davey, M.D., &c., Hanwell County Lunatic Asylum. With an appendix of Strictures on the Periodical Press. London: H. Baillière, 1843. 8vo, pp. 54.

A Practical Treatise on the Diseases peculiar to Women, illustrated by cases derived from hospital and private practice. By Samuel Ashwell, M.D., Guy's Hospital. Part. II. Organic Diseases. London: S. Highley, 1843. 8vo, pp. 260.

A Practical Treatise on the Diseases of the Testis and of the Spermatic Cord and Scrotum, with engraved illustrations. By T. B. Curling, London Hospital. London: Longman and Co., 1843. 8vo, pp. 542.

The Plea of Humanity and Common Sense against Surgical Operations for the Cure of Impediments of the Speech. By James Wright, Esq. London: Sherwood and Co., 1843. 8vo, pp. 51.

Journal of the Statistical Society of London for May, 1843. London: J. W. Parker.

A Letter to the Rev. Dr. Warneford on the Collegiate Plan of Providing for Pupils at the Birmingham Royal School of Medicine and Surgery. By the Rev. V. Thomas. Printed for private distribution at Oxford, 1843. 4to, pp. 54.

CORRESPONDENTS.

The statements of *J. G. H.* are anonymous. Moreover, we believe that the patient on whom the mesmerist employed his art was neither more nor less than a regular humbug. The roughest terms are fittest for describing such actors.

The papers of *Dr. Ayres* and *Dr. Hocken* will be inserted.

Cyclopædia of Anatomy.—We are requested by the Editor of the Cyclopædia of Anatomy and Physiology to state that part of that work is in a very forward state and may be expected early next month.